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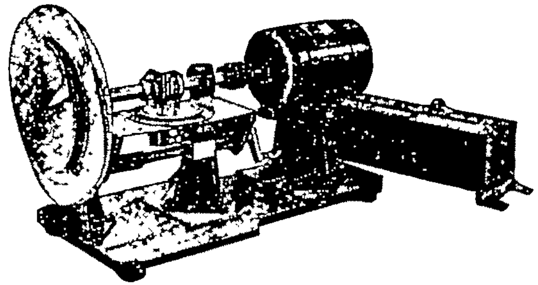

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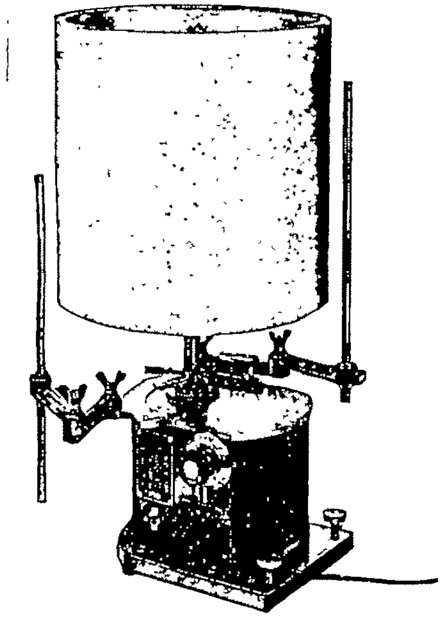

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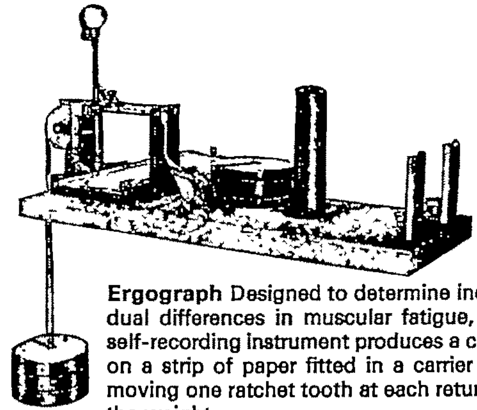

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RESPONSE LATENCY IN THE SERIAL LEARNING OF SHORT LISTS

By ALAN KENNEDY
Department of Psychology, University of Dundee

CALCU

The relationship between serial position and response latency in the learning and recall of short lists of letters was examined. For a given length of list the relationship was non-linear, responses to the first and final positions being faster in general than to other positions. Response-latencies regardless of serial position tended to be longer for a seven-item list than for a five-item one. Rate-of-presentation was not a relevant variable within the limits of the experimental conditions employed.

INTRODUCTION

The time taken to name an object varies in a linear fashion with the log frequency-of-occurrence of the name (Oldfield & Wingfield, 1965). Oldfield (1966) has argued that neither a simple binary choice selection through a word store, nor (obviously) a *seriatim* scan could lead to a relationship of this form. A memory store structured so that access-time is related to familiarity appears to be suggested: in particular, Oldfield has put forward the notion of a two-process search in which part of the naming-time for a given object is taken up in identification as to relative familiarity, and part in scanning through a store.

There is close agreement from a number of sources that response latency in a verbal learning task continues to decline after response probability has reached a maximum (i.e. during over-learning). Cofer & Musgrave (1963, p. 330) have reviewed much of the evidence for this, and further reference may be made to Williams (1962) and Brown & Huda (1961).

These results are quite consonant with an incremental view of learning. For example, in Hullian learning theory, latency is an index of response strength along with amplitude, probability and resistance to extinction. With certain forms of manual skill it is reasonable to expect latency of decrease as motor responses become more practised. In the case of tasks with only minimal motor involvement, though, latency changes reflect changes in some central processing system. Here the Hullian model does not go far in giving insight into possible mechanisms underlying the process.

A position which regards recall as some kind of search process (e.g. Yntema & Trask, 1963) forces one to look on the changes in latency during over-learning as resulting from some change in the accessibility of items in memory. This is not incompatible with the position taken by Oldfield *et al.* that reductions in naming-time (accessibility) follow from increases in familiarity of an item. A list of words to be learned in this view can be seen as a special case of Oldfield's more general analysis. As learning proceeds the list will come to occupy an increasingly dominant or prior position—that is, it will become very familiar. If items are chosen to be of roughly equal (and low) familiarity initially, and further, if the list is short so that all items are acquired during the first trial or two, then difficulties posed by particular items in a list being at different levels of familiarity will be avoided.

The fact that once learning is under way a list of words comes to be seen by a subject as isolated from his total store of words has been often mentioned (e.g. Underwood & Schulz, 1960; Postman, 1963). The usual finding is that intrusions seldom occur from extra-experimental sources. If a well-learned serial list of this sort may be looked on as occupying a single 'equi-frequent' band in store, then, given an appropriate latency measure, the mode of retrieval from memory may be inferred from the time taken to search for particular serial positions. An approach to the problem of assessing retrieval strategy of this general type has been proposed by Sternberg (1966). He found that the time taken to indicate if a given 'probe' item was a member of a previously learned series or not increased as the length of the series increased. However, since times to indicate positive and negative instances did not differ greatly he argued that even when an item was present in a series the whole list was scanned. The analysis of response latency from different serial positions provides a clear test of this contention.

The present study measured response latency during the over-learning of short serial lists of letters. Two lists of five and seven letters were used. Retrieval-time in a recall test was also measured: single probe items were presented to the subject with instructions to respond with the next item in the already-learned list. Two rates of presentation were used with the expectation that this variable would influence efficiency during learning but not during recall.

METHOD

Subjects and apparatus

Subjects were twenty-four volunteers drawn from first- and second-year psychology students at Queen's. All were unfamiliar with the experimental procedure used here.

Two tape-recorders were used: one to present lists to the subject and one to record the experimental session. The tape from the second recorder was processed later by playing it into a voice key. Onset of a stimulus item started a timer, and the subject's response stopped it.

Design

Two distinct experiments were in fact carried out: five- and seven-letter lists being examined each at two rates of presentation. However, by extracting four positions for analysis from the seven-letter list (those representing the beginning, middle and end), both lists could be analysed together in a 2 × 2 × 4 factorial design with rate-of-presentation, list-length, and serial position as factors. The serial positions selected from the five- and seven-item lists of course allow only an approximate comparison rather than a precise one. Four groups of six subjects were assigned to the four combinations of list-length and rate-of-presentation. Within a cell three groups of two subjects learned the three serial orders.

Procedure

A list of five consonants (j, k, p, q, v) and a list of seven consonants (j, k, l, p, q, s, v) were chosen and recorded, each in three different orders. These lists have roughly equal and moderately high acoustic confusability (Conrad, 1964). The letters satisfied the further important requirement of reliably triggering the voice key when spoken by both male and female voices with varying clarity and loudness. For each serial order the lists were recorded at 3 sec and 5 sec presentation rates for ten trials. The interval between trials was in each case twice the presentation rate. In addition to these lists two series of 'probe' items—one for each list length—were also recorded. These were single letters presented at 8 sec intervals, testing each serial position three times in a random fashion.

Standard instructions for serial learning were given, subjects being told to respond verbally, as quickly as possible, anticipating the next item in the list. A signal tone preceded each trial. No response was demanded from the presentation of the last item in a list, although some subjects volunteered one during the early trials. A criterion of five consecutive correct trials was used. The learning session was followed after a short interval (never more than 2 min) by the recall test. Here subjects were told they would hear a list of letters read very slowly, and as each appeared they were to respond as quickly as possible with the next letter in the list they had just learned. They were told that letters would appear randomly with regard to serial position, and that each letter would appear 'several times'.

The recording of a test session was processed later and a latency measure obtained for responses made during acquisition and recall.

RESULTS

Acquisition

Results from the three orders of presentation were combined.

During acquisition no response was made to the final item in the lists. Response latencies to the signal tone were not included in the analysis, since the tone was not used as a 'probe' item during recall. A $2 \times 2 \times 4$ analysis of variance was carried out comparing mean performance on the last three of the five trials following the learning criterion. This used data from the four tested positions of the five-item list and positions 1, 3, 4 and 6 of the seven-item list and revealed serial position to be the only significant factor ($F = 3{\cdot}09$; D.F. 3, 60; $P < 0{\cdot}05$). Mean latency over these positions for all subjects was 1·24, 1·32, 1·27, and 1·14 sec. Responses to the final position were significantly faster ($P < 0{\cdot}05$) than to other positions. The apparent curvilinear relationship was not supported statistically, the difference between positions 1 and 2 just failing to achieve significance. The predicted interaction between rate of presentation and serial position was not significant ($F = 1{\cdot}85$; D.F. 3, 60).

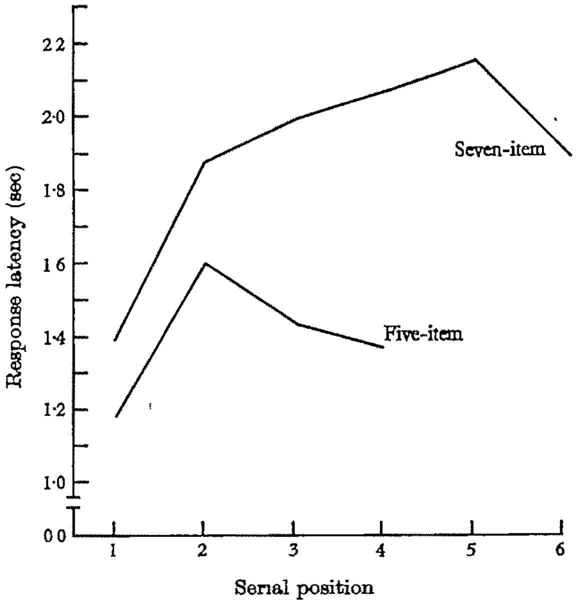

FIG. 1. Mean response latency at each serial position of five- and seven-item lists.

Recall

Four errors in recall were noted; in each case these were replaced for the purposes of analysis by the mean latency at that serial position for the particular subject. More elaborate statistical precautions seemed unnecessary in view of the very small number of cases.

The mean latencies to 'probe' items testing each serial position for the two lengths of list are shown in Fig. 1. It is clear that, at least for the shorter list, the relationship is not linear. An analysis of variance for this list-length only gave serial position as a significant main effect ($F = 6{\cdot}64$; D.F. 3, 30; $P < 0{\cdot}001$). Positions 1 versus 2, and 2 versus 4 differed significantly ($P < 0{\cdot}05$). In the case of the seven-item list latency appears to increase across serial position, with the exception of the final position. This tendency is supported statistically: F for position is 4·51 (D.F. 5, 50; $P < 0{\cdot}01$). Latency for position 1 is shorter than for all other positions; position 2 is shorter than position 5; and positions 5 and 6 differ significantly (all $P < 0{\cdot}05$). Comparisons between treatment means in all cases were made using the Newman–Kuels procedure (Winer, 1962).

A 2 × 2 × 4 analysis of variance was carried out over the four positions tested in the acquisition analysis. Significant factors were list length ($F = 6{\cdot}95$; D.F. 1, 20; $P < 0{\cdot}025$) and serial position ($F = 8{\cdot}72$; D.F. 3, 60; $P < 0{\cdot}001$). Clearly response latencies are longer in recalling from the longer list.

DISCUSSION

In the case of lists of items which are greatly over-learned, and are acquired in one or two trials, serial position effects of the sort shown here obviously may not be ascribed to the results of differential practice at different positions.

The analysis of serial position shown in the present results gives some support for Sternberg's findings, if it can be accepted that similar retrieval strategies would be employed in dealing with material presented to both the visual and auditory modalities. Such a position is clearly suggested by Conrad's (1964) work, and more recently Wickelgren (1965) has gone as far as to suggest tentatively that short-term memory for both modalities may be considered to be the auditory subsystem of long-term memory.

In a recent paper Sternberg (1967) has examined position as a variable in serial scanning and concluded that the relationship between serial position and latency is linear. In a seven-item list he found latency to vary in a linear fashion from about 900 msec for position 1 to over 1300 msec for position 6. Sternberg's experimental procedure did not involve the use of over-learned lists and consequently the error rate for longer lists was very high (*c.* 25 % for seven-item lists). For this reason it is doubtful if a comparison with the present study is useful, although it should be noted that for a proportion of his subjects the relationship between position and latency was either 'flat' or curvilinear.

In the processing of serial lists the final position appears to have a special significance. This is explicitly built in to information-processing theories of the serial-position-effect like that of Feigenbaum & Simon (1962). When a list is short this has the effect of providing a curvilinear relationship between latency and position similar to that for error measures.

If the argument is accepted that short lists learned for many trials can be thought of as occupying single 'equi-frequent' bands in store then the present results give some indication that items are retrieved from such bands by means of a serial scan, but that priority is given to the ends of lists over other positions. It may well turn out that in this case the ends of lists are merely instances of what Feigenbaum & Simon (1962) term 'perceptually unique anchor points', and that equal priority could be given to items distinguished in any special way (i.e. the von Restorff effect).

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THE CHOICE OF THE PASSIVE VOICE IN A COMMUNICATIVE TASK

By P. N. JOHNSON-LAIRD

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Subjects had to rank-order four logically equivalent but syntactically different sentences in terms of their appropriateness as descriptions of one diagram in contrast to another. A prediction of this rank order was derived on the assumption that subjects would attempt to emphasize the larger areas of colour in the stimuli, and that passive sentences emphasize the logical object to a greater extent than active sentences. Four groups were run in a 2 × 2 design. One variable was whether the stimulus to be described was symmetrically or asymmetrically divided into two colours. When one type of stimulus was to be described the other type served as a contrast. The other variable was whether the larger area of the asymmetrical stimulus was denoted by the logical subject or the logical object of the sentences. These two variables gave rise to a subsidiary prediction concerning the degree of correlation between the predicted rank orders and the actual performance of the groups. Both predictions were confirmed.

Introduction

An earlier report (Johnson-Laird, 1968) presented evidence to support an hypothesis about the difference between the active and passive voice in English. The hypothesis was that the passive implies that the logical object is more important than the logical subject, whereas the active implies that there is a minimal difference in the importance of these two entities or that the logical subject slightly predominates. The notion of 'importance' is vague and any attempt at a precise definition would be premature. In framing rather similar hypotheses, Jespersen (1924) talks of 'the centre of interest', and Frege (1879) of 'what we want the hearer to attend to specially'. Clark (1965), reporting the results of a study in which subjects completed active and passive sentence-frames, claims that 'people put what they want to talk about...in the beginning of the sentence'. There are also several technical notions which reflect similar preoccupations on the part of linguists; but nowhere is there an adequate definition. Perhaps an example may clarify matters: according to the hypothesis, 'the man was kissed by the woman' implies that it is the man (logical object) who is important, whereas 'the woman kissed the man' implies that there is little difference between the importance of the two or that the woman (logical subject) is slightly more important.

Evidence supporting the hypothesis was provided by the original experiment in which students had to draw diagrams to illustrate an active sentence and its corresponding passive. The present experiment tested the same hypothesis but used the converse task: students were presented with a diagram and had to choose an appropriate description for it from a set of active and passive sentences. In fact, they made a succession of choices yielding a rank order of preference for the sentences. This task bears some resemblance to the speaker's performance in *choosing* a sentence to describe something; the earlier task bears some resemblance to the listener's performance in *interpreting* a sentence.

In English, as in many languages, the grammatical subject usually precedes the grammatical object in the word-order of sentences. When the logical object is brought to the front of the sentence, as with passives or sentences of the form 'he is easy to please' (cf. Chomsky, 1965, p. 222), it becomes the grammatical subject. There are, as Chomsky points out, stylistic inversions which allow the logical object to come to the front of the sentence without any change in its grammatical status, e.g. 'him I really like'. Such inversions seem rather rare, and it is likely that they occur merely as a way of emphasizing the logical object when recourse to the passive is inappropriate. It would be rather ponderous to say, 'he is really liked by me': pronouns rarely feature in the agent-phrase of the passive (Svartvik, 1966). (It is tempting to suggest that this is because pronouns are used to refer to what is the centre of interest.)

It has been hypothesized that the passive is used to emphasize the logical object. But is this emphasis due to it becoming the grammatical subject of the sentence or to it being placed prior to the logical subject? It seems likely that word-order is the more crucial factor—why else should the stylistic inversions have the same effect? In order to test this conjecture, the subjects made their choices from a set of sentences of the type shown in Table 1. The transformational derivations of these four sentences are similar (cf. Katz & Postal, 1964, p. 73). They are derived basically from two underlying structures (strictly speaking, the passive has a slightly different underlying structure from that of the active) which may be represented in simplified form as: (*a*) There is a blue area. (*b*) A blue area precedes a red area. Their main semantic import stems from structure (*b*), in which it is clear that 'a blue area' is the logical subject and 'a red area' is the logical object. ('A blue area' is *not* the logical object of structure (*a*), which does not possess one (Chomsky, 1965, p. 72).) The *Normal active* in Table 1, like most normal actives, has its logical subject prior to its logical object. Similarly, the *Normal passive* has its logical object prior to its logical subject. The *Inverted* sentences invert these customary word-orders but without changing the grammatical status of the noun-phrases. These sentences were paired with two types of stimulus so that, if the emphasis of the passive were due to word-order, the preferred description for one stimulus would be the Normal passive and the preferred description for the other stimulus would be the Inverted passive. If, on the other hand, the emphasis were due to grammatical function, the preferred description for both stimuli would be the Normal passive, since there would be no purpose in choosing the more cumbersome Inverted passive.

Table 1. *An example of one set of sentences used in the experiment*

1. There is a blue area that precedes a red area.	(Normal active)
2. There is a red area that a blue area precedes.	(Inverted active)
3. There is a red area that is preceded by a blue area.	(Normal passive)
4. There is a blue area that a red area is preceded by.	(Inverted passive)

The *situation* of a communication (its socio-physical setting and linguistic context) probably exerts a decisive influence upon the form it takes and the way that it is understood, e.g. Wason (1965) has demonstrated an effect of setting on the evaluation of negative sentences. It is also likely that the appropriateness of a description depends to some extent upon what is *not* to be described, i.e. the contrast class of those things which the description is intended to rule out. Clearly, it is desirable to control this, otherwise subjects will set up their own imaginary contrast classes. In the present experiment, two types of stimulus were used: one divided symmetrically into areas of red and blue and the other divided asymmetrically into the same two colours (cf. Fig. 1). When one type was to be described the other type served as a contrast. This introduced a problematic aspect into the task, since the sentences are true descriptions of both stimuli. But if voice has the emphatic function that has been hypothesized, the task is solvable. Moreover, if human communication is rational, the order of preference for the descriptions of one type of stimulus should be exactly the opposite to that for the other type of stimulus. Similarly, there should be a comparable degree of concordance between the orders obtained in the two conditions. These two hypotheses, which may be subsumed under the general heading of 'the rationality assumption', seemed unlikely to be confirmed in full and predictions about departures from them will be made subsequently.

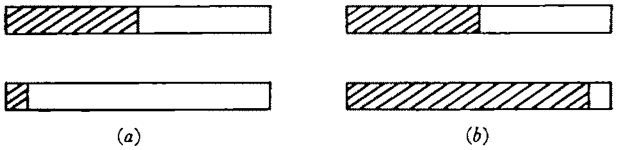

Fig. 1. Two examples of the pairs of stimuli. The shaded area represents blue, the unshaded area represents red.

METHOD

Task

Each subject was presented with two stimuli which were long narrow rectangles, one divided symmetrically and the other asymmetrically into two coloured areas, as shown in Fig. 1. A set of four sentences was also presented; an example of one set is given in Table 1. All the sentences specify the arrangement of colours depicted in both stimuli, but the task was to rank-order them in terms of their appropriateness as descriptions of one stimulus as opposed to the other. The communicative aspect of the task was enhanced by telling the subject, prior to his first choice, that 'someone else' had to decide which of the two stimuli was being communicated, on the basis of this choice.

Design

Although each subject was his own control for the test of the basic hypothesis, four independent groups were run in a 2 × 2 design. The first variable was designed to test the word-order hypothesis. The sentences and stimuli were paired so that the larger area of the asymmetrical stimulus was denoted either by the logical object or the logical subject of the sentences. The second variable was designed to investigate the rationality assumption: the stimulus to be described was either asymmetrical or symmetrical. The combination of these two variables produced the following four groups:

AO: the asymmetrical stimulus had to be described and its larger area was denoted by the logical object.

AS: The asymmetrical stimulus had to be described and its larger area was denoted by the logical subject.

SO: the symmetrical stimulus had to be described and the larger area of the asymmetrical stimulus was denoted by the logical object.

SS: The symmetrical stimulus had to be described and the larger area of the asymmetrical stimulus was denoted by the logical subject.

Predictions

The predicted rank order of choices for AO was (1) the normal passive, (2) the inverted active, (3) the normal active, (4) the inverted passive.

The Normal passive emphasizes the larger area of the asymmetrical stimulus, which is denoted by the logical object. The Inverted active similarly brings the logical object to the front of the sentence but is *ex hypothesi* less emphatic than the passive, so it should be the second choice. The third choice follows from the simple corollary that an inappropriate emphasis is less pronounced with an active than with a passive.

For AS, it is the logical subject which denotes the larger area of the asymmetrical stimulus. Hence, the first choice should be the Inverted passive, which emphasizes the logical subject. By similar reasoning, the rank order for AS should be exactly the opposite to that for AO.

It follows from the rationality assumption that the best description of the symmetrical stimulus would be the one which was the worst description of the asymmetrical stimulus, and so on. The main prediction for the symmetrical groups was thus that their rank orders would be exactly the opposite to those of their corresponding asymmetrical groups.

These rank-order predictions should apply if the materials and subjects were ideal. Neither of these conditions was likely, so a subsidiary prediction was made. This concerned the *degree* of correlation between actual performance and the predicted rank orders, and it was based upon two intuitive assumptions. Firstly, the material was not ideal: an informal test revealed that the Inverted passive was considered slightly bizarre and was harder to interpret correctly. Subjects in AS and SO might be reluctant to make it their first choice. Secondly, the symmetrical groups might adopt strategies other than the 'rational' one predicted. C. Alexander (personal communication) has shown that symmetrical stimuli are considered to be simpler than asymmetrical ones. Similarly, actives seem to be simpler than passives. So, one alternative strategy was that subjects might make a first choice of an active sentence, and base their choices throughout upon a criterion of simplicity. Since this was a factor likely to affect strategy as a whole rather than the choice of a particular sentence, it would have a greater effect upon the diversity of performance than the 'difficulty' of the Inverted passive. It was thus possible to predict a trend over the groups in terms of the degree of correlation between actual performance and the main prediction. AO would have the highest correlation because it was unaffected by either factor; AS would have the next highest correlation because it was affected only by the difficulty of the Inverted passive; SS would be next because it was affected by the strategy factor; and finally, SO would have the lowest correlation because it was affected by both factors. The main and subsidiary predictions are summarized in Table 2.

Table 2. *Summary of the design, the predicted rank order of preference for each group, and the predicted trend of the degree of correlation over the groups*

(The upper row of each cell states the predicted rank order of sentences for the group, and the lower row states the place of the group in the predicted trend of the degree of correlation between performance and the main prediction. Note: 1 = Normal active, 2 = Inverted active, 3 = Normal passive, and 4 = Inverted passive.)

	The part of the sentences denoting the larger area of the asymmetrical stimulus	
Stimulus communicated	Logical object	Logical subject
Asymmetrical	AO: 3214	AS: 4123
	1st	2nd
Symmetrical	SO: 4123	SS: 3214
	4th	3rd

Subjects

The subjects were thirty-two undergraduates (sixteen male, sixteen female) in the Department of Psychology, University College London. They were native-speakers of English and ignorant of the purpose of the experiment.

Materials and procedure

Four separate sets of sentences were used in the experiment. One set is shown in Table 1, and another set was constructed from it by switching round the two colour-names in each sentence. This set was used with stimuli which were constructed by switching round the colours of the stimuli shown in Fig. 1. As a mutual control for any directional bias of the verbs, a further two sets of sentences were constructed using the verb 'follows' instead of 'precedes'. All the sentences were typed in block capitals on separate slips of paper.

Three strip stimuli were prepared on separate cards so that, by suitable rotations and arrangements, each of the required pairs could be assembled. Each of the single stimuli consisted of a strip, 5 × ½ in., drawn on a plain 6 × 4 in. card. One was divided symmetrically into a red and a blue area, and two were divided asymmetrically in a ratio of 1:11, with the larger area being red in one case and blue in the other.

Each of the four sets of sentences and each of the four pairs of stimuli were used in every group. There were, in fact, just four permissible combinations in any group, since each set of sentences required a different stimulus-pair so as to satisfy the group conditions. Two subjects were run for each combination in order to counterbalance the vertical arrangement of the stimuli. There were thus eight subjects in each group. The sentences were also arranged vertically, using four basic arrangements derived from a Williams square. These four arrangements were used twice in a group: once for the sentences with 'follows' and once for the sentences with 'precedes'.

The subjects were tested individually and allocated in rotation to their particular set of materials. They sat at a table on which lay the stimuli and sentences, covered by a blank piece of paper. This was removed and the following instructions were read: 'All these statements are true descriptions of both these drawings. I want you to check, for your own satisfaction, that this is so.' When the subject had announced agreement, and some subjects disagreed at first, having misinterpreted the Inverted passive, the experimenter continued: 'Imagine that you're to try to communicate to someone else that you're looking at *this* stimulus [here, the experimenter pointed to the relevant stimulus] rather than this one, which one of the statements would you choose as most likely to convey this information? Remember that the person you're communicating with knows that it's one or other of these two stimuli, but he doesn't know which one. So, it's your task to try to make this clear by selecting what you feel is the most suitable statement for *this* stimulus.' Here, again, the experimenter pointed to the relevant stimulus. No explicit mention was made of the fact that 'follows' and 'precedes' related to the conventional left-to-right direction. This, of course, was implicit in the opening sentence of the instructions, and was not queried by any subject.

After the subject had made a first choice, the chosen sentence was removed from the array and the experimenter asked, 'Now, which of *these* (sentences) would you choose as the most suitable?' This procedure was repeated for the subject's third choice.

RESULTS

The subjects' three choices yielded a rank order of preference for the four sentences; the proportion of times each type of sentence occurred in each rank is shown for the separate groups in Fig. 2. The difference between performance with 'follows' and 'precedes' was minimal, and all the analyses were on pooled data. In the test of the combined significance of the thirty-two independent rankings, Kendall's tau was $+0{\cdot}30$ ($P < 0{\cdot}001$); thus, the main prediction was confirmed. Separate values for each group were also calculated: for AO, $\tau = +0{\cdot}71$ ($P < 0{\cdot}00003$); for AS,

$\tau = +0{\cdot}46$ ($P < 0{\cdot}006$); for SO, $\tau = +0{\cdot}21$ (n.s.); and for SS, $\tau = -0{\cdot}17$ (n.s.). This trend over the groups is in accord with the subsidiary prediction and a trend test (Jonckheere, 1954), using subjects' S values as data, was significant ($P < 0{\cdot}003$).

The numbers of times the asymmetrical groups selected the two types of passive on their first choice are shown in Table 3. The two groups were compared in order to test the word-order hypothesis, and the deviation from proportionality was significant on a Fisher–Yates exact test ($P = 0{\cdot}001$, one tail).

The low correlations in the symmetrical groups could have been caused by the subjects adopting some common alternative to the predicted performance. Kendall's coefficient of concordance was 0·21 for SO and 0·14 for SS, and neither coefficient was significant. This indicates that no clear alternative stategy was adopted by these groups, a fact readily observable from Fig. 2 (*c*) and (*d*).

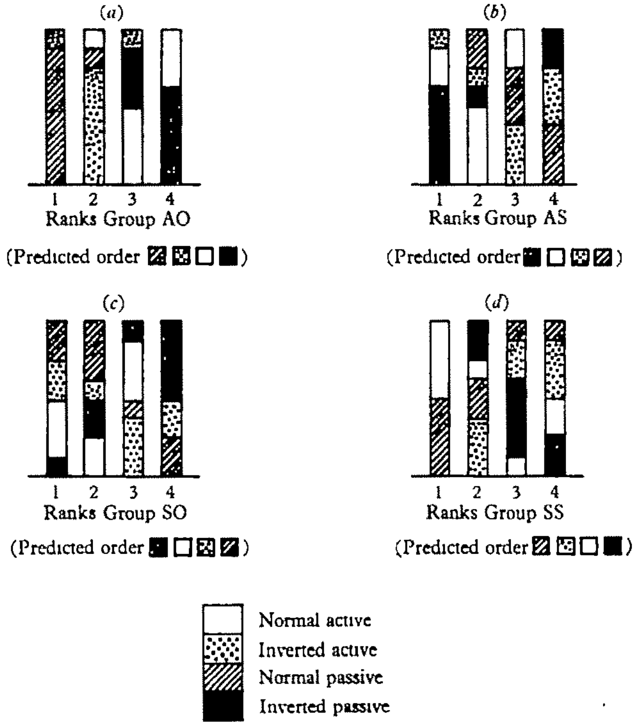

Fig. 2. The proportion of times the four types of statement occurred in the ranks yielded by the subjects' preference choices. There were eight subjects in each group. (The areas within each column are arranged so that, as one moves up each column, they should diminish in size as a function of the subjects' adherence to predicted performance.)

Table 3. *The number of times the two types of passive sentence were chosen first by the asymmetrical groups: there were eight subjects in each group*

	Type of passive chosen first	
	Normal	Inverted
Group AO	7	0
Group AS	0	5

Qualitative results

Group AO. Four out of eight subjects performed exactly as predicted. Seven subjects reported that they had attempted to emphasize the larger area of the asymmetrical stimulus but they admitted that this had been difficult, especially on the second and third choices. The remaining subject claimed that his choices were motivated by the ease of understanding the sentences. Only one subject initially thought that the Inverted passive was a false description of the stimuli: he and another subject mentally eliminated it before making their first choice.

Group AS. Two subjects performed exactly as predicted. Again, the majority reported that they had attempted to emphasize the larger area of the asymmetrical stimulus. Two subjects said that they had ignored the stimuli initially and 'considered only what the statements had conjured up', 'treated the statements in a vacuum'; one performed as predicted and the other nearly so. Two subjects refused to choose the Inverted passive, one because it was 'confusing' and the other because it was 'ungrammatical'.

The introspective reports from the symmetrical groups were somewhat disparate, reflecting a wider variety of strategies and some dissatisfaction with the task: it was 'frustrating' and 'impossible'. Only three subjects stated that they had taken into account what was the best description of the contrast stimulus.

Group SS. There were signs of the expected conflict in this group; as can be seen from Fig. 2(*d*), the first choice was equally divided between the Normal passive and the Normal active. The choice of the active was justified mainly on the grounds of simplicity. The choice of the passive was explained on several grounds, of which the most interesting and coherent was that it implied that the colour denoted by the logical object was larger in the symmetrical stimulus than the area of the same colour in the asymmetrical stimulus.

Group SO. It was obvious from the subjects' reports that they had found the task difficult; indeed, some subjects were unable to say anything about what had motivated their choices. SO was also the only group where each of the four types of sentence was chosen at least once as the best description (cf. Fig. 2*c*). The single report common to a number of subjects was that they had rejected the Inverted passive because it was difficult to understand.

DISCUSSION

The correlation between performance and the main prediction was small, positive, and highly significant. But this result is slightly misleading because only the asymmetrical groups show significant correlations with their predictions. The group results do in fact strongly conform to the predicted trend, with the performance of the symmetrical groups showing a greater diversity rather than the adoption of a common but unexpected strategy.

The experiment shows that a passive sentence is not necessarily chosen to describe the same things as its corresponding active. It confirms the basic hypothesis about the difference between the two voices, in a situation where relative size determines what has to be emphasized. The passive, as hypothesized, is more emphatic than the active, perhaps because a greater effort is required to handle it (cf. McMahon, 1963; Slobin, 1966). It is the verb-phrase that indicates which voice is being used, and hence the level of emphasis. But it is word-order that indicates to what the emphasis is being given. This is supported by the preference for the Normal passive in AO and the inverted passive in AS. Word-order alone, however, is insufficient to account for the results since it fails to explain the preference for the passive rather than the active. The results thus support both the voice factor and the word-order factor; in everyday discourse, these two factors are, of course, usually confounded.

Could it be that the results are an artifact and that subjects merely chose the sentence in which the two noun-phrases occurred in the same left-to-right order as the colours of the stimuli? Although this again fails to explain the preference for the passive in the asymmetrical groups, it has an initial air of plausibility. However, consider what happened in AO: two subjects received the stimuli illustrated in Fig. 1 (*a*) and the sentences shown in Table 1; they should, according to this alternative hypothesis, choose the Inverted passive or the Normal active. In fact, neither of these sentences were chosen by them. It is a simple matter to show that there should be no difference between the first choices of AO and AS, if this alternative hypothesis were true. Half the subjects in both groups should choose the Inverted passive, and the other half should choose the Normal passive—assuming that the choice of the actives has been eliminated on some grounds. The actual difference between the two groups was highly significant and satisfactorily eliminates this possible explanation.

It was predicted that the pattern of choices for the symmetrical stimulus would be more diverse than for the asymmetrical stimulus, but the extent of the actual difference was unexpected. Certainly, human communication departs from the rational ideal in this situation: few subjects realized that in choosing a description for one stimulus it was a valuable strategy to consider its suitability as a description for the contrast stimulus. This finding is reminiscent of the well-known inability of subjects to adopt indirect methods in reasoning about problems. It implies that there may be gross differences in the ease with which something may be communicated even within a limited universe of discourse.

The comparison of the two symmetrical groups is less revealing. A plausible explanation of the performance of SO, which is supported by the introspections, is that the conflict between strategies lowered the subjects' tolerance for the Inverted passive. Since the sole difference between the conditions for SO and SS is in their respective contrast stimuli, the findings are consistent with the notion that *situation* exerts an influence upon the form of a communication.

It seems likely, having confirmed the basic hypothesis in both an interpretative task and a choice task, that voice could be used to communicate information in an experiment using one subject in the speaker's role and another subject in the listener's role. A more important task, however, is to generalize the findings to a greater variety of verbal material, and one such study, with promising preliminary results, is nearing completion.

Controversy about the synonymity of the active and passive voice has been carrying on for some years. On the basis of linguistic intuition, there have been arguments for synonymity (e.g. Katz & Postal, 1964) and against it (e.g. Chomsky, 1957; Ziff, 1966). The present empirical study suggests that both these 'all-or-none' alternatives may be wrong. The voices differ in their emphatic pattern and this may yield differences in meaning. This is particularly so for ambiguous sentences involving quantifiers, where active and passive may have different privileged interpretations, e.g. 'Some girls are liked by all the boys'; 'All the boys like some girls'. These two sentences are ambiguous in the same way, but the more probable interpretation of the first is that all the boys like some *particular* girls, and the more probable interpretation of the second is that all the boys like (some or other) girls. Change the order of the two quantifiers, and it becomes clear that surface word-order rather than voice *per se* is responsible for the difference in the privileged interpretations.

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BODY-SWAY SUGGESTIBILITY AND SEMANTIC SATIATION

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Two tests of semantic satiation and a body-sway test were administered to fifty post-graduate students. Satiation scores for words in subjects' native language were found to be highly correlated with body-sway ($r = 0{\cdot}801$), while satiation for words in English were not so related. The correlation between satiation and body-sway was consonant with similar relationships observed between hypnosis and satiation. Some hypotheses regarding the nature of semantic satiation and primary suggestibility are advanced.

In a previous study, individual differences in hypnotic susceptibility were found to be positively correlated with resistance to semantic satiation (Das, 1964*a*). Since hypnosis is induced and maintained through repetitive verbal suggestions, it was argued that an individual prone to lose the meaning of words on repetition could not attain a deep hypnotic trance. In the body-sway test, just as in hypnosis, a few verbal suggestions are repeated over and over again. These words may be conceived of as conditioned stimuli evoking the postural sway, which would then be a conditioned response. The present author has viewed hypnosis as a multiplicative function of conditioning and inhibition. Hypnosis, according to this, has been described as a conditioned (learned) state of selective cortical inhibition (Das, 1959). The explanation may be extended to body-sway suggestibility. If the state of selective inhibition is attained through the comprehension of the meaning of the words used in suggestion, it seems reasonable that those who satiate quickly would show poor suggestibility.

In the present experiment, an attempt at obtaining a correlation between body-sway suggestibility and semantic satiation has been made by administering, first, the test of satiation and then body-sway to adult subjects. An additional objective of this study is to test the effect of satiated words as stimuli for suggesting sway.

METHOD

Subjects

Fifty male students of the M.A. classes of the university acted as subjects. None of them were taking their degrees in psychology.

Design and procedure

The experiment may be described in three parts, as these were given to the subject in a 50 min session. In the first part, a standard semantic satiation task was administered to the subject. Then in the second part, which almost immediately followed the first, the subject was given the body-sway test, In the third part, words used in suggesting sway were satiated, following which the body-sway test was readministered.

Semantic satiation was measured as before (Das, 1964*a*, 1966) following the polarity difference method. The Oriya (native language of the subject) synonyms of CHILD, ME, TRUTH, RICH, HOUSE, were presented to the subject in one of five predetermined random orders. The subject was required to rate all the words, first, on three evaluative semantic differential scales (Osgood, Suci & Tannenbaum, 1957), then repeat a word as fast as possible for 15 sec at the rate of 2–3 repetitions per sec, and immediately, after that, rate that word on one of the three scales provided by the experimenter. Thus after the initial ratings, each word was repeated and rated three times in a random order, each time for one scale. Satiation score was computed by subtracting the pre-satiation rating from post-satiation rating as indicated by Lambert & Jakobovits (1960). A negative score indicated satiation while a positive score indicated generation of meaning. Thus a high score indicated resistance to satiation.

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The body-sway test consisted of 1 min of static ataxia followed by 2 min of suggestion. The suggestions (you are falling forward, falling, falling forward, etc.) were given by the experimenter in English in the usual manner, asking the subject to imagine that he was standing on the top of a cliff and was falling forward. Maximum forward and backward sway were recorded. The instrument for measuring sway was a simple device (see Das, 1960) in which the subject's forward sway displaced one of the two small sliding plates of aluminium mounted on a ruler to the left, and his backward sway displaced the other plate to the right. The plates maintained their positions unless moved further by a subsequently greater sway, thus enabling the experimeter to record a subject's maximum sway in either direction.

The third part of the experiment was given approximately 3 min after the second part. The three English words, *You*, *Falling*, *Forward*, were satiated essentially in the same procedure as the five Oriya words. Immediately after satiation, the subject was administered 2 min of body-sway suggestions, without static ataxia. It was assumed that the satiation treatment might weaken the stimulus value of the three suggestion words, and the degree of such weakening could be seen in the extent of decrement of a subject's post-satiation sway.

RESULTS

Maximum forward and backward sway could alone be recorded in the method used here to measure body-sway. Thus each subject had four basic scores: maximum forward and backward sway in static ataxia, and maximum forward and backward sway under suggestion. The subject's score in static ataxia and suggested sway were the highest sway in inches, irrespective of the direction of sway. This is referred to as maximum sway in the remaining part of the paper. Besides these, for each subject a total sway (maximum forward plus maximum backward) difference between maximum suggested sway and static ataxia, and difference between the maximum sways before and after satiation treatment were computed. During suggested sway, a fall was scored as 8 in. Unless specifically stated, all sway measures are from the test given before the English words were satiated (pre-satiation sway).

Two satiation scores, one for the five Oriya words and the other for the three English words, were computed for each subject. The Oriya satiation words were considered as appropriately reflecting stable individual differences in satiation, and were expected to be correlated with sway in the same manner in which these were previously correlated with hypnosis (Das, 1964*a*).

Table 1 gives the product–moment correlations obtained between several measures of body-sway and semantic satiation. All three measures of suggested sway have positive correlations with satiation of Oriya words. Of these, the strongest correlation is obtained for the sway score representing the net effect of suggestion after subtracting the sway under static ataxia. This is as it should be, since individual differences in static ataxia might be responsible for an underestimation of the relationship of satiation with the other sway scores. The English satiation scores, however, seem to have no predictive value for extent of body-sway ($r = 0{\cdot}13$).

The effect of directly satiating the English words on subsequent body-sway in which these words were used appears to be minimal. The pre- and post-satiation sway means were 2·47 and 2·23 in. respectively, hardly differing from each other

(t = 0·56, D.F. = 49). The individual differences in English satiation did not also correlate with those in post- minus pre-satiation sway (r = 0·055). Oriya satiation scores, similarly, showed insignificant relationship with the difference between post- and pre-satiation sways (r = −0·103). Thus the direct satiation of English suggestion words had little effect on post-satiation sway, which is further supported by a high consistency in the subject's sway before and after satiation (r = 0·822). If satiation had any disrupting effect such consistency would not be obtained.

Table 1. *Product–moment correlations between semantic satiation and body-sway measures* (N = 50)

	r	P
Satiation (Oriya) vs.		
Suggested sway – static ataxia	0·801	0·01
Suggested sway	0·364	0·01
Total suggested sway	0·347	0·02
Post – pre-satiation suggested sway	−0·103	N.S.
Satiation (English) vs.		
Suggested sway – static ataxia	0·13	N.S.
Post – pre-satiation suggested sway	−0·055	N.S.

Satiation scores

A comparison of the satiation scores for Oriya and English words showed that the latter was positive, implying a greater resistance to satiation than the former. The mean satiation per word rated on three scales was −0·052 (S.D. 1·35) for the native language and +0·126 (S.D. 1·24) for the foreign language. However, the mean difference was not statistically significant.

Although the means are small, it would not be correct to assume that there was no change in the subject's rating following the repetition of a word. In fact, larger positive and negative scores were obtained from the sample, which when summed algebraically give a near-zero mean. The relatively large S.D.s indicate a somewhat wide range of satiation scores. However, the means are small which are characteristically obtained in bilingual samples such as ours (Jakobovits & Lambert, 1961; Das, 1964*a*). There was a low but significant positive correlation between subjects' Oriya and English satiation scores (r = 0·283; P = 0·05), which might have interesting implications for satiation research on bilinguals. For the present study, low correlation is consistent with the disparity in correlations between suggested sway on the one hand, and English and Oriya satiation on the other.

DISCUSSION

One of the objectives of the present study was to examine if the same relationship between hypnosis and satiation obtained previously could be obtained for body-sway and satiation. This seems to be clearly the case as seen from the high correlation between the two (r = 0·801). It compares favourably with an r of 0·667 (N = 62) between hypnosis and satiation (Das, 1964*a*). A major implication of the result may concern our understanding of the nature of hypnosis and body-sway suggestibility. Both must be manifestations of some common and essential process. As suggested earlier in this paper, both could perhaps be described as a multiplicative function of a learning and an inhibition component: the verbal stimuli evoking specific conditioned responses in the subject, and their monotonous repetition facilitating the development of cortical inhibition. The author had suggested that semantic satiation may be more appropriately related to the learning component of hypnosis (Das, 1964*a*). The same relationship seems to be operative for the learning component of body-sway suggestibility.

Here, one is forced to distinguish semantic satiation from other forms of satiation which are essentially sequels to the subject's exposure to merely repetitive stimuli. The latter has the same effect as mild sedatives, generally lowering a subject's arousal level. Whereas a slight *increment* in suggestibility is expected following such satiation (see Evans, 1967), a *decrement* in suggestibility is found to result from semantic satiation. In order to understand this disparity, the author (Das, 1964*b*) has proposed an attention hypothesis which holds that a decrement in attention to the word may occur with its repetition. Assuming that any meaningful word elicits a specific orienting response, it is suggested that on repeated presentation of the word devoid of its semantic associations or its attendant changes in the environment, such orienting response may not be evoked. A familiar word, such as those used here, gives rise to a clear expectation of consequent events upon its presentation. The lack of such events is easily detected during repetitions of the word under the satiation procedure. The subject has no need to be attentive to such words. Semantic satiation, according to the hypothesis, may be viewed as the adapting out or extinction of the orienting response. Thus, individuals who are highly susceptible to this kind of adaptation would be minimally responsive to the repetitive suggestions of the body-sway test, as also to hypnotic suggestions. For the same reason, they would be poor conditioners in verbal conditioning tasks, as shown before (Das, 1966).

Another objective of the present study was to attempt to reduce post-satiation sway by satiating the English suggestion words. This was not realized; the pre- and post-satiation mean sways were hardly different. It was noticed that the English words, on the other hand, had a positive mean satiation score, indicating at least no great loss in attention evoked by them. These findings may be accounted for by the fact that resistance to satiation for words in a foreign language is expected from past studies (Jakobovits & Lambert, 1961). Further, the English words had already been repeated several times during their use in the 2-min pre-satiation sway test; thus further satiation was minimal.

In conclusion it may be observed that the relationship consistently obtained between objective behavioural measures such as semantic satiation and conditioning on the one hand and hypnosis and body-sway on the other throws some light on the nature of each of these phenomena. It also offers a rational basis for the psychometric classification of hypnosis and body-sway as measures of primary suggestibility (Eysenck, 1947). Evans (1967) has recently accused the researchers in the field of suggestibility of supplying evidence mostly on the 'psychometric properties of the tests than (on) the phenomena they measure' (p. 147). This paper is a modest attempt at correcting that situation.

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AN EXPERIMENTAL COMPARISON OF RECTILINEAR AND CURVILINEAR ILLUSIONS

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The magnitudes of the illusory distortions apparent in the Ponzo figure and in three newly devised spatial patterns are estimated. Comparison of these with an appropriate control situation indicates that significant spatial distortions remain present in configurations which do not necessarily define angular contexts. Implications of these findings for 'the perspective theory' of illusions and for 'the carpentered world hypothesis' are discussed.

The appearances assumed by certain two-dimensional line-drawings are contrary to their physical forms. It is customary to refer to these as 'geometrical illusions'. Most persuasive of the many theories formulated in the attempt to explain the distortions seen in illusions is that which invokes the influence of perspective elements embodied within their configurations. This theory was first proposed by Thiéry (1896) who drew attention to the similarity of some illusions to two-dimensional projections of concrete objects and three-dimensional scenes. The best-known and most widely studied of all illusions, that first described by Müller-Lyer in 1889, reminded Thiéry of the cross-member and trestle legs of a saw-bench. The closeness of this resemblance led him to suggest that familiarity of meaningful objects and real-life scenes might conceivably be influential in inducing distortion in certain two-dimensional drawings.

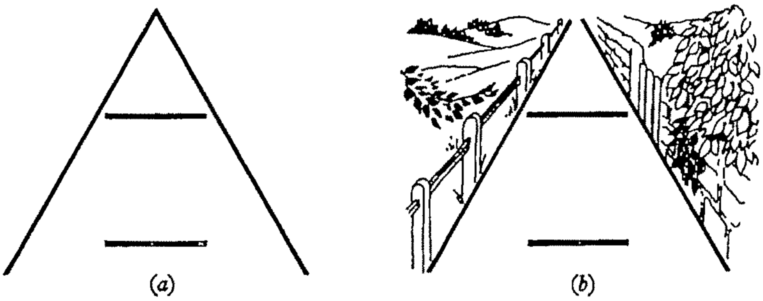
(*a*) (*b*)

Fig. 1. The Ponzo illusion and a perspective projection of a typical three-dimensional scene.

The illusion to which the perspective theory should be most obviously relevant appears in the Ponzo (1912) illusion, reproduced in Fig. 1 (*a*). This pattern is defined by two horizontal lines enclosed within an inverted V-shaped bracket. Although the two horizontal lines are identical in length, under the influence of the illusion, the upper line appears to be appreciably longer than the lower. The Ponzo figure is strongly reminiscent of projections of such scenes as road-ways, or railway-tracks, the lines representing which appear to converge as they recede into the distance. Lines superimposed upon perspective drawings of three-dimensional scenes are distorted in the same way as those which form part of the Ponzo figure. An example of distortion of this kind apparent in a two-dimensional drawing of a typical three-dimensional scene is illustrated in Fig. 1 (*b*). Here, as in the conventional Ponzo figure, the upper line appears to be somewhat longer than the lower.

The importance of the perspective theory extends beyond its original premise. Rivers (1901, 1905) observed indigenous Torres and Todas Indians to be differentially susceptible to illusory distortions in comparison with subjects raised in Western cultures. He proposed that these differences arise directly as a consequence of the experience of the spatial features of typical environments. More recently the findings of Rivers have been reaffirmed in the long-term cross-cultural studies undertaken by Segall, Campbell & Herskovits (1963, 1966), their formulation of '*the carpentered world hypothesis*' being expressed as follows: '... *In the carpentered Western world such a great proportion of artifacts are rectangular that the habit of interpreting obtuse and acute angles as rectangular surfaces extended into space is a very useful one.... In a culture where rectangles did not dominate, this habit might be absent.... Another cultural factor which might be related to illusions is two-dimensional representation of three-dimensional objects. Perspective drawing is a most persuasive feature of Euro-american culture. It is a substantial feature of the visual world from childhood on. Children in this culture, from a very early age, attempt to make representations of this kind themselves. The techniques or conventions involved may be related to the habits of interference which some illusions illustrate*....' (*op. cit.*, p. 71). The view expressed here may be understood as a logical extension of that suggested by Thiéry and the results of the extensive studies reported by Segall *et al.* testify strongly to its validity.

In the majority of experimental studies of illusions rectilinear figures have been employed. Thiéry and others attempted to extend the implications of perspective by considering the Müller-Lyer (1889), Ponzo (1912), Poggendorff (1860), Hering (1861) and Wundt (1896) figures specifically in these terms. Relatively few attempts have been made to evaluate the distortions apparent in curvilinear figures, however, and, as far as the writer is aware, no such figures have been studied cross-culturally. The purpose of the present paper is to initiate consideration of the possibility of illusory spatial distortions being apparent in figures other than those with which they are usually associated.

METHOD

As applied to the Ponzo and other illusions the explanatory principle of the perspective theory rests upon the similarity between the angular components of two-dimensional patterns and projections of scenes and structures actually having extension in three-dimensional space. Being apparently situated at different distances, the two horizontal lines embodied within the Ponzo figure appear to differ in size, the farther line seeming to be longer than the nearer. In Fig. 2 the two horizontal lines of the Ponzo illusion have been placed in contexts which are other than angular. Figs. 2 (*a*) and (*b*) are defined by curved rather than straight lines, and Fig. 2 (*c*) by two pairs of vertical bars. Inspection of each of these figures suggests that an illusion similar in nature to that seen in the Ponzo figure is clearly evident. Accordingly, the problem becomes that of measuring the extents of these illusory distortions.

Apparatus

An apparatus was constructed embodying two horizontal slots seen as white bars against a black background. These slots were 5 mm wide and they could be adjusted in length from 70 to 130 mm symmetrically about their centres. The background against which the slots were placed was 610 mm square. The angle, the two sets of vertical bars, the concave and the convex patterns were painted in white line upon sheets of a transparent plastic material. When these were placed against the background the slots could be seen clearly through them. Thus, the over-all patterns assumed by the background and the displays superimposed upon it closely resembled the three forms illustrated in Fig. 2 and the conventional Ponzo illusion shown in Fig. 1 (*a*). A further sheet of plastic omitting any lines was introduced also, this providing the appropriate control situation.

Procedure

A total of 100 individual subjects participated in this experiment. Twenty-three of these were university students ranging in age from 18 to 27 years, eight of whom were female. The remaining subjects were selected essentially at random from the populace of Newcastle upon Tyne.They ranged in age from 19 to 69 years, forty-three being female. All subjects had normal vision. They were shown the five displays individually, the upper horizontal slot being presented in a standard length of 100 mm. In each case subjects were asked to adjust the lower slot to such a position that it appeared to be of the same length as the upper. Initial positions of the lower variable slot were presented in a randomly determined order throughout the whole range of possible adjustment. All subjects made ten adjustments in relation to each of the five displays, the order of presentation of which was randomized appropriately. This order was changed for each subject participating in the experiment.

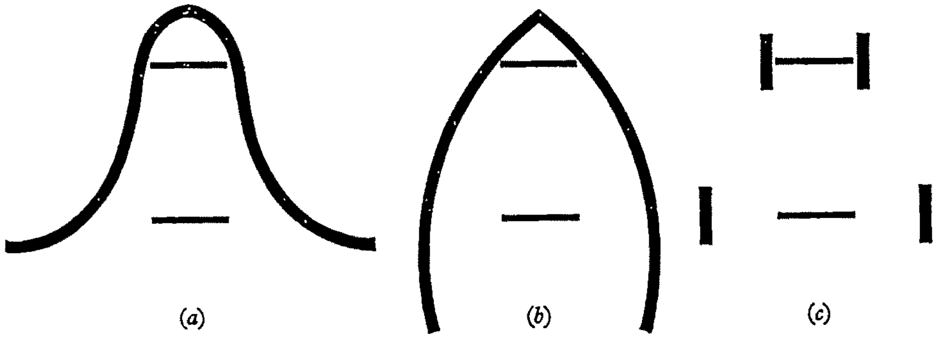
(*a*) (*b*) (*c*)

Fig. 2. Three new Ponzo-type illusions.

Table 1. *The extent to which each lower horizontal line was adjusted so as to appear of the same length as the upper*

Display...		1 (*a*)	2 (*a*)	2 (*b*)	2 (*c*)	Control
Adjustment of lower line	$\bar{X}$	13·44	12·57	10·85	8·57	0·37
	$\sigma_{\bar{X}}$	0·52	0·41	0·31	0·38	0·35

RESULTS

The results of this experiment are entered in Table 1. Numerical values, expressed in units of mm indicating the extent to which each lower line was adjusted to a length in excess of the upper, are shown for all four experimental displays. Alongside these are the data referring to adjustment of the variable line in the control condition. The mean values ($\bar{X}$) refer to the combined judgements of the total sample of subjects, each of whom made ten judgements. The standard errors of estimate ($\sigma_{\bar{X}}$) associated with the mean values are entered beneath them. Sophisticated statistical treatment of data of such clarity is almost gratuitous. They show that an illusory spatial distortion, of the kind usually associated with the conventional Ponzo figure, became apparent in both curvilinear displays and in the pattern which embodies two pairs of vertical bars. The mean values of all these distortions considered against their standard errors indicate clearly that in a statistical sense each differs from the control situation in which no additional contour was present. Accordingly, it may be concluded that illusory distortion in the relative apparent lengths of two horizontal lines embodied within the contexts of further contours has been demonstrated.

DISCUSSION

The explanatory features of the perspective theory rest upon the assumption that the organization of the perceptual mechanisms underlying the appearance of illusions is initiated and developed as a consequence of experience of features of three-dimensional space. Illusory distortions become evident in line-drawings prepared upon two-dimensional surfaces. Certain line-drawings resemble plan-, front- and side-elevation projections of three-dimensional objects and solid structures. In the physical world, different parts of such structures are located at different distances in relation to the eyes of an observer. Accordingly, the retinal images due to them differ in size. In general, those parts which appear to be closer are larger than others which are seemingly more distant. Certain perceptual scaling mechanisms are brought into operation when judging the sizes of objects in relation to their distances. The central and most important concept of the perspective theory is that these same mechanisms operate when observing two-dimensional patterns. Unfortunately, the characteristic functional modes of these spatial scaling mechanisms fail to be appropriate when they operate in such a way as to extrapolate information from some line-drawings. When this proves to be the case an illusion arises. Accordingly, it may be argued that, according to perspective theory, the price paid by the visual projection system for veridical perception of three-dimensional space is consequential illusory distortion of two-dimensional space.

As referred to in introducing the topic of this paper the perspective theory has been invoked in a variety of different ways since it was first proposed. It provides the only theoretical rationale for those cross-cultural studies which have shown people living in variously structured environments to be differentially susceptible to illusions. The only serious challenge to the theory was made by Gregory (1966) who sought, first to demonstrate it to be invalid since the prediction which it makes of the nature of the distortion expected to become apparent in the Ponzo illusion seems to be the reverse of that usually seen (according to Gregory, on the perspective theory, the upper horizontal line in the Ponzo figure should appear shorter not longer than the lower) and, secondly, to replace it with his version of the Tausch (1954) theory according to which illusions arise as a consequence of the inappropriate operation of the mechanism responsible for size-constancy. Elsewhere, Fisher (1967) has suggested that this contrary view proceeds from mistaken assumptions; specifically taking up the point of the expected nature of the Ponzo illusion on a perspective interpretation of its form and concluding that the characteristic appearance of this particular illusion is in no way at variance with the perspective theory.

It is debatable also whether, or not, the results of the experiment reported here may be interpreted as confounding the perspective theory. The conventional Ponzo illusion undoubtedly contains strongly defined features which are reminiscent of scenes having extension in depth—that is to say it resembles a two-dimensional projection of a three-dimensional spatial pattern. The curvilinear patterns might conceivably be interpreted in a similar way, and in solid form the upper part of each of these would usually be located at a somewhat further distance than the lower. A perspective interpretation of the form of two pairs of vertical bars is possible also but it is perhaps somewhat less likely. Certainly, if present at all, any depth features apparent in this figure are minimal in comparison with the others. Nevertheless, a significant illusory distortion of lines placed within them undoubtedly remains clearly evident.

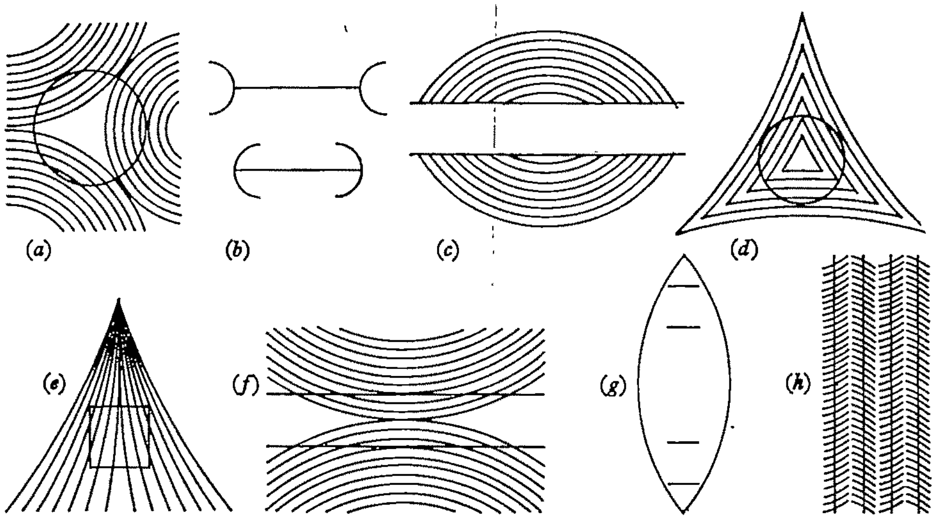

Fig. 3. A number of well-known illusions modified so as to include curved rather than straight lines.

As described above, the hypothesis of the 'carpentered world' rests upon the same theoretical foundation as that of 'the perspective theory'. Invoked by Segall *et al.* it implies that we (Westerners) should be more susceptible to illusions which arise in rectilinear figures since we have somewhat greater experience of straight and angular patterns than those who dwell in more primitive environments. The ecologies of the societies studied by Rivers, by Segall *et al.* and by many others concerned to delineate the cultural determinants of perception, however, were liberally endowed with naturally occurring curvilinear objects. Moreover, such primitive peoples as these are extremely familiar with a variety of curved artifacts, with circular-section structures and with curvilinear projections.

The distorting components of the curvilinear patterns used in this experiment may be interpreted as projections of forms having extension in depth. They may, however, also be understood as representing structures more familiar to those dwelling in primitive societies than our own. Accordingly, the argument from the characteristic rectilinear nature of typical Western environments seems somewhat questionable. This is reinforced by the distortions apparent in each of the eight patterns illustrated in Fig. 3. These are all modified forms of otherwise well-known illusions. The distorting aspects of the original versions of these patterns are all defined by straight lines. Here these have been replaced throughout by curves. The rectilinear form of

Fig. 3(*a*) was illustrated by Luckiesh (1922), (*b*) by Müller-Lyer (1889), (*c*) by Wundt (1896), (*d*) by Gatti (1926), (*e*) by Ehrenstein (1924), (*f*) by Hering (1861), (*g*) is a 'double' version of one of the curvilinear patterns studied in this paper and (*h*) was described by Zöllner (1860). The similarity of the distortions evident in these figures with those in the original illusions is such that it is difficult to believe that they should not be closely related. If this were the case then it would almost certainly follow that the perceptual scaling mechanisms responsible for their being at variance with their denotable physical forms either are the same or operate in similar ways. Hence, we are forced to the conclusion that the carpentered world hypothesis, along with other theories which appeal to the influences of typical depth features in inducing illusory spatial distortions, requires reconsideration.

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RETINAL INHIBITION IN VISUAL DISTORTION

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Objections to Gregory's theory of the visual distortions and illusions are reviewed and an alternative theory involving inhibitory processes at the retinal level is proposed. This theory, however, deals only with distortions and not with illusions.

A paper by Gregory (1963) claimed that perceptual illusions and distortions could be explained in terms of what he calls 'inappropriate constancy scaling'. This is the tendency, where there is evidence of distance, for the visual system to enlarge perceptually objects that appear more distant. It is the well-known regression to the real object. This gave rise to a good deal of controversy, which can be divided into that concerning illusions and that concerning distortions. The distinction made here between illusions and distortions is illustrated by the difference between the Müller-Lyer and Ponzo figures (Fig. 1) and the rest of the illustrations. The distortions involve perceptual bending or tilting of lines, whereas the illusions involve a perceptual alteration of size. The difficulty of applying Gregory's theory to the distortions is that there are cases where the direction of the perspective is a matter of opinion yet the distortion is always the same (Wallace, 1966; Brown & Houssiadas, 1965). Difficulties of applying Gregory's theory to the illusions occur because it is difficult to find perspective in some figures at all, yet there is an illusion (Day, 1965; Hotopf, 1966); or because there are cases of clear perspective but no illusion (Humphrey & Morgan, 1965). Gregory's case is that constancy scaling can be set by apparent distance as in Emmert's law (this he calls 'secondary constancy scaling'). It can also be set directly by perspective features of the display even though these are at variance with apparent distance, i.e. the display is seen by the viewer to be drawn on a plane surface. This Gregory calls 'primary constancy scaling', and this, in his view, is the cause of illusions and distortions. The perspective content of the figure need not even enter the viewer's consciousness. The mechanism by which illusions and distortions occur is an expansion of the part of the figure that appears most distant. With this in mind, let us consider in more detail the difficulties mentioned above.

Wallace's (1966) objection to the theory concerns the distortions in the Hering and Orbisson figures (Figs. 6*a*, 2). He states that both these figures look like receding tunnels, and thus that if the distortion in the Hering figure is as predicted, that in the Orbisson figure is not. In his reply Gregory (1966*a*) appears to agree with Wallace on the perspective of the Hering figure, but he does not agree on that of the Orbisson figure. He claims that this decision about which perspective to accept is not arbitrary, and that his device for gauging the depth effect of flat figures (Gregory, 1966*b*) shows figures to have depth effects which correctly predict distortions. Thus his device would show that the Hering figure was seen as a receding cone, and the distortion is as would be predicted. But in his reply to Wallace, Gregory states that his device shows the Orbisson figure to be seen as flat. One can achieve either of the two perspective effects by varying the relative spacing of the circles, and these are seen

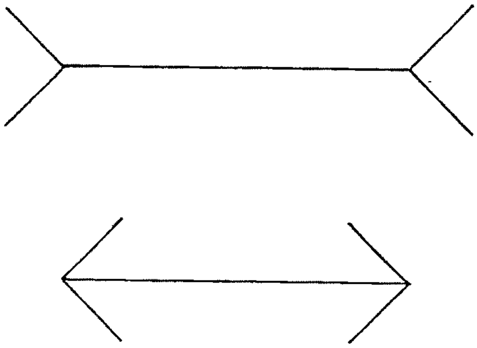

Fig. 1 (*a*). Müller-Lyer figure.

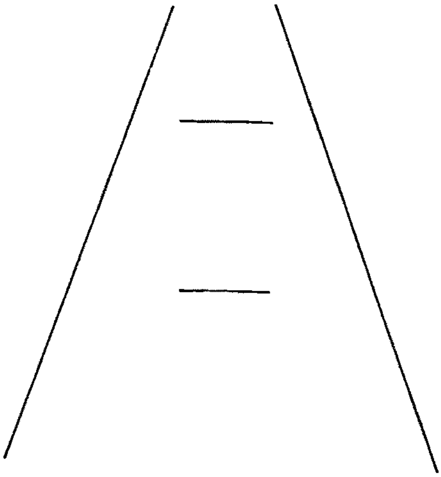

Fig. 1 (*b*). Ponzo figure.

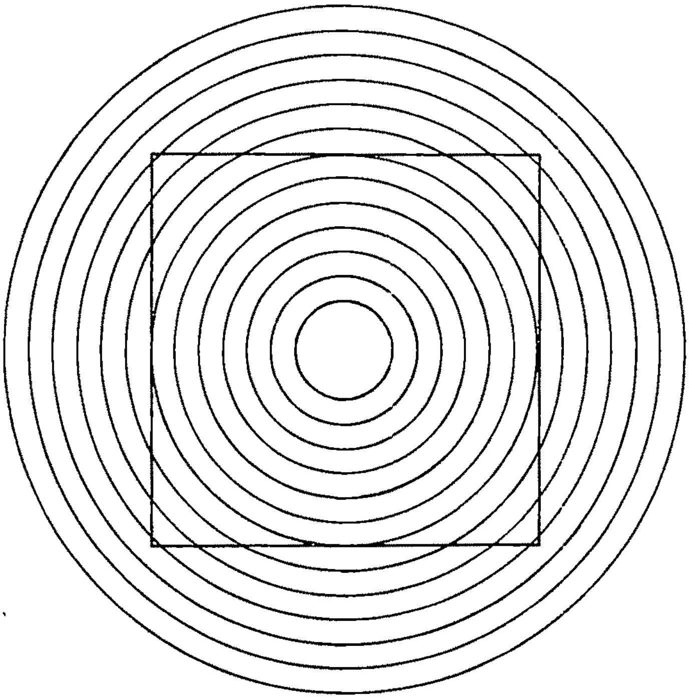

Fig. 2. Orbisson figure.

as the corresponding depth effects on Gregory's device. However, the distortion remains the same whether the figure is seen as receding, protruding or flat. Gregory goes on to explain the distortion in terms of a version of the Müller-Lyer illusion. It is difficult to see this as the typical perspective, and it seems that to justify his theory, Gregory must ignore the obvious perspective of the figures and seek a much more complex and abstruse perspective. With the obvious perspective, his theory will not work.

It seems to the writer that this gives strong support to the claim made by Humphrey & Morgan (1965) that: 'A theory which appeals to the idea of automatic compensation for unconsciously perceived depth is in obvious danger of being irrefutable.' Gregory's 'primary constancy' scaling is difficult to demonstrate independently of the illusions.

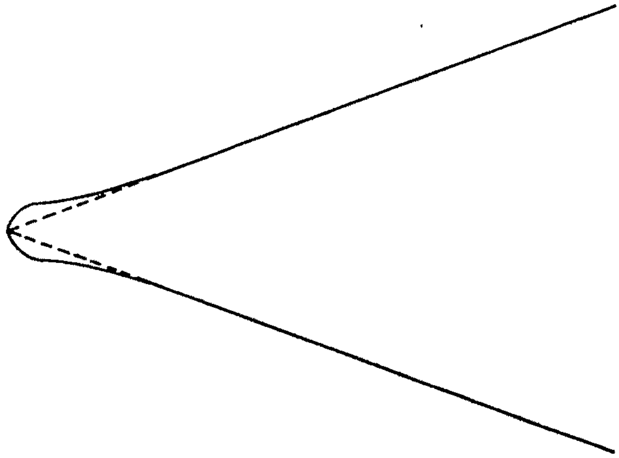

Fig. 3

Humphrey & Morgan also suggest a different way of thinking about the role of constancy scaling in the distortions which gives rise to slightly different predictions from Gregory's theory. This is to imagine that, where the background suggests a plane in depth, the distorted lines are being seen as drawn on that plane. These lines will then be distorted in the direction in which they would have to be curved when *actually* drawn on such a plane if they were to give rise to a straight-line image on the retina. This again is a fairly straightforward case of regression to the real object. Thus in the case of a cone with the point nearest the observer, to appear as straight on the retina, a line drawn on the surface of the cone would have to bow towards its point. In the case of a cone with its point farthest from the observer, to appear as a straight line on the retina, a line drawn on the surface of the cone would again have to bow towards its point. The bowing of the lines is 'correct' for either perspective in the case of the Orbisson figure, but is 'wrong' for either perspective in the Hering figure. Perspective seems irrelevant. This version of a constancy theory will not work either.

Papers by Ganz (1964, 1966) suggest, but do not elaborate, a way in which retinal inhibition might bring about the illusions. This would overcome the difficulties of explanations in terms of constancy. The present writer can see such an explanation applying to the distortions, but not to the illusions. Such an explanation of the distortions requires small extra assumptions, but seems to be worth considering. Ganz reviews work showing that contours are sharpened perceptually by processes of inhibition within a certain distance of their location on the retina. By such processes of inhibition he accounts for the apparent displacement of lines in figural after-

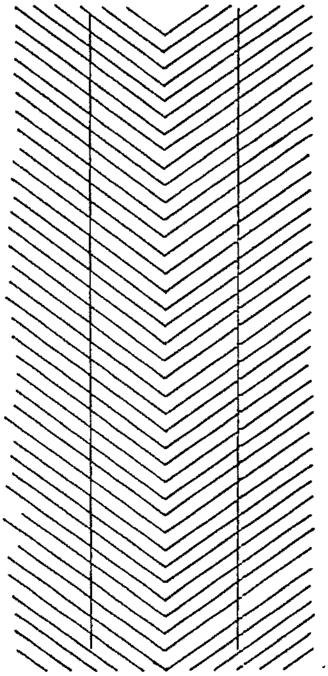

Fig. 4(*a*). Zöllner figure.

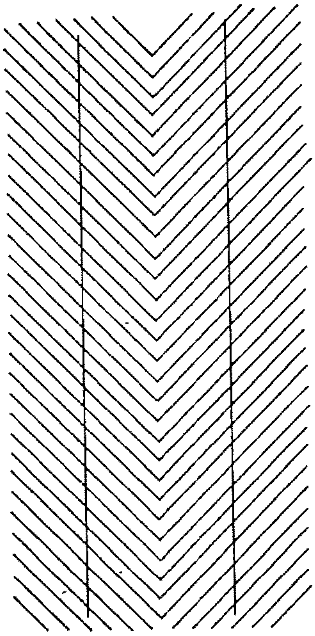

Fig. 4(*b*)

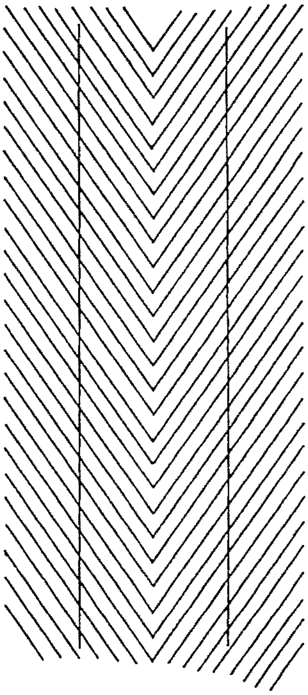

Fig. 4(*c*)

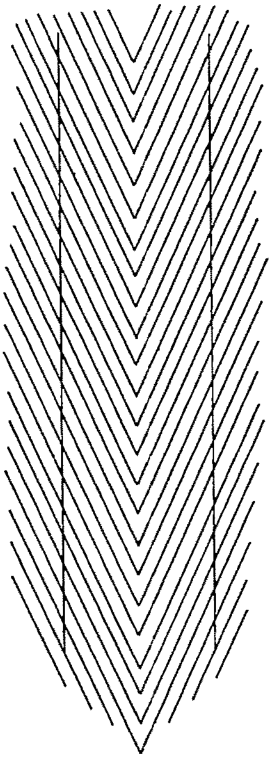

Fig. 4(*d*)

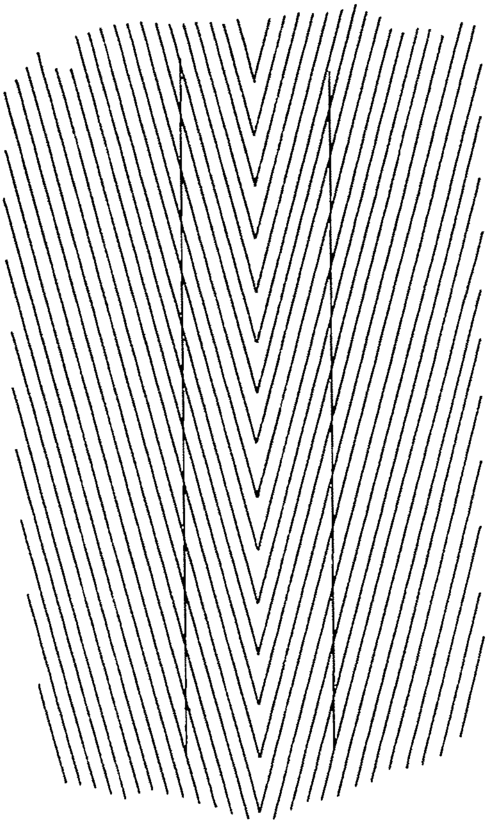

Fig. 4(*e*)

effects and also mentions that the displacement found in the visual distortions might be similarly explained.

The displacement effect reaches a maximum at a small distance from the contour and is progressively reduced from this maximum in either direction. This is the result of eye movements. When the image is stabilized on the retina, the point of maximum displacement is found to be much closer to the inducing contour (Ganz, 1966). In the theoretical case of a perfectly stabilized image, Ganz claims that the maximum displacement would be at the inducing contour. Thus, according to this theory, where the lines of an angle run close together (as they do in an acute angle) they should give rise to a very slight displacement of each other. This would give a perceptual effect something like that shown in solid lines in Fig. 3. This is the enlargement of acute angles suggested by Wallace (1966) and several other writers.

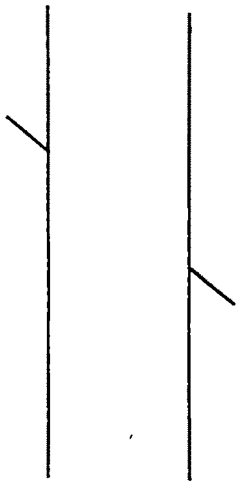

Fig. 5(*a*). Poggendorff figure.

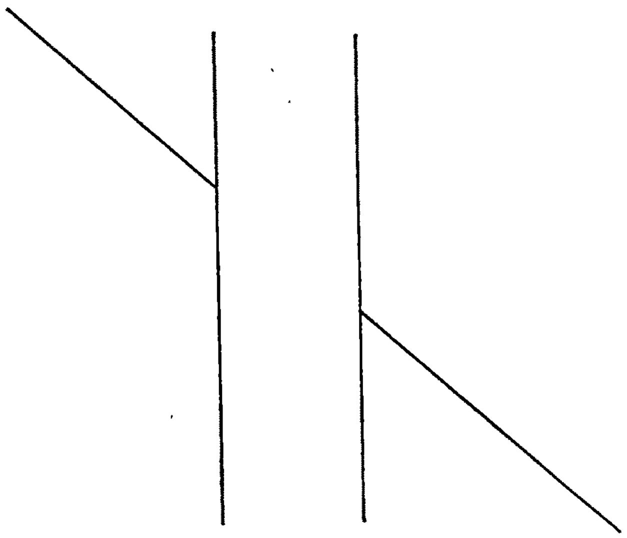

Fig. 5(*b*)

To extend this principle to the distortions the extra assumption is required that, where many lines take one direction and few another, these latter will be most affected by perceptual displacement. In general distortion will appear where there is least evidence countermanding it. The Zöllner figure (Fig. 4) illustrates this clearly. Each crossing line will displace a small segment so as to give the effect, if the angle of incidence of the multiple lines is not too acute, of rotating the single line. If the angle of incidence is very acute, the single line will even lose its integrity and appear to be a stepwise line. Both these effects may appear to the reader in Fig. 4. It is also worth pointing out that the version of the Zöllner figure which gives the clearest impression of depth (Fig. 4*a*) is the one which shows little or no distortion. Again perspective seems irrelevant.

The Poggendorff figure (Fig. 5) can be seen as the converse of the Zöllner figure. If the vertical lines were to be affected by distortion derived from the angles they would have to be perceptually bent. All the distortion therefore appears on the slanting line where there is less evidence to countermand distortion.

Another small point is illustrated by Fig. 5. The reader may agree that the distortion of the short lines (Fig. 5*a*) is greater than that of the longer lines (Fig. 5*b*). This is further evidence that the distorting effect comes chiefly from the parts of the lines close to the junctions with other lines and that the greater the evidence against the distortion (i.e. the longer the slanting line), the less distortion there will be.

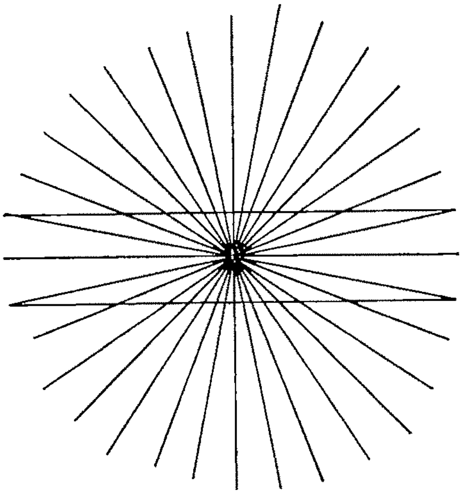

Fig. 6 (*a*). Hering figure.

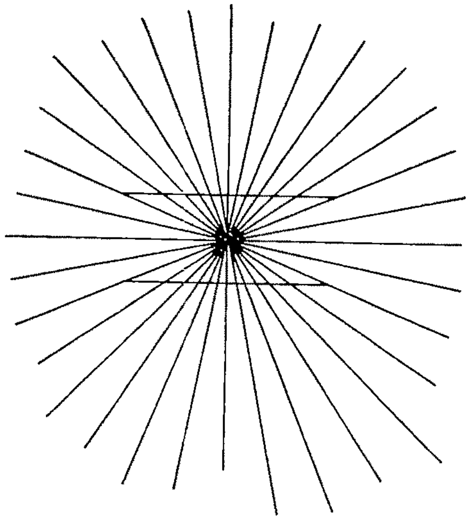

Fig. 6 (*b*)

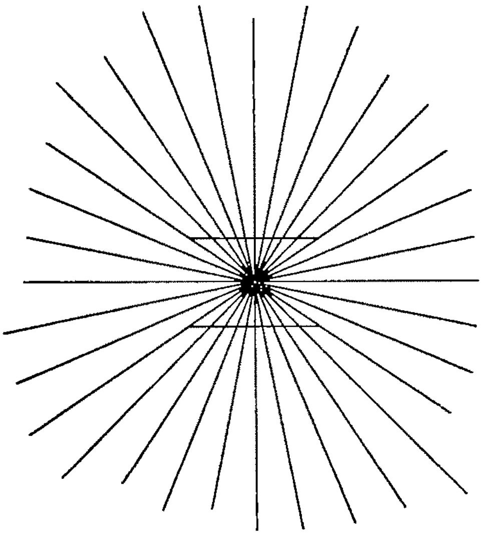

Fig. 6 (*c*)

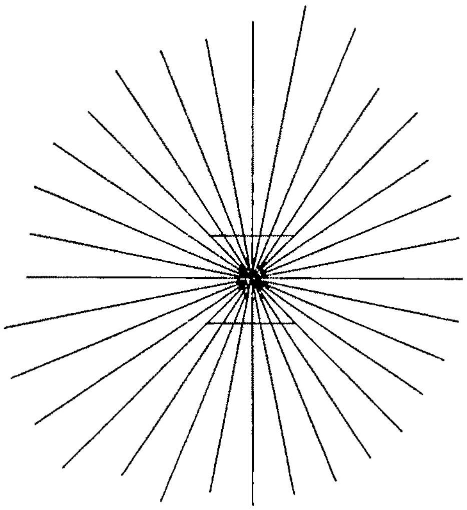

Fig. 6 (*d*)

The various versions of the Zöllner figure suggest that the angle of the crossing lines is an important determinant of the amount of distortion experienced. Another instance of this can be seen in the Hering figure (Fig. 6). Distortion does not occur until the straight lines cross radiating lines at an angle which is very acute. Only when the straight lines are extended fairly well out does distortion occur. Yet one would suppose that there is still sufficient evidence of depth for distortion to be produced by Gregory's theory.

Those displays where the background forms an obvious 'picture' in depth make Gregory's explanation very appealing, but Brown & Houssiadas (1965) present evidence that, if the 'picture' background is composed of dots and not lines, there is no distortion. One is forced to the conclusion that the immediate appeal of Gregory's theory is misleading.

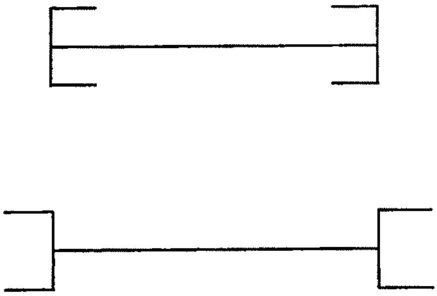
Fig. 7 (*a*)

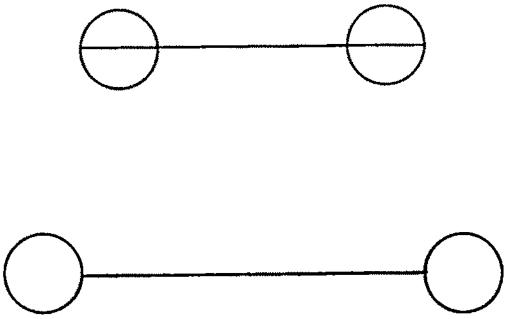
Fig. 7 (*b*)

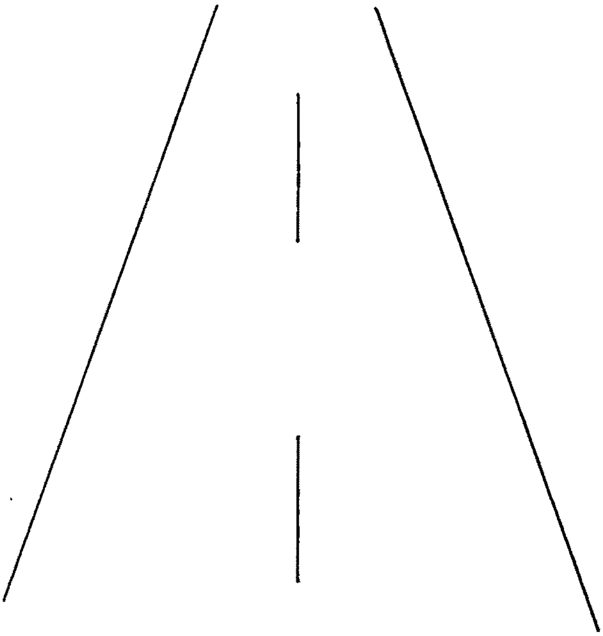
Fig. 8

Retinal inhibition can thus account for distortions, but not the illusions (e.g. (Müller-Lyer and Ponzo). Gregory's theory neatly accounted for both, but even in the case of the illusions, there are difficulties which discourage its acceptance. Day (1965) and Hotopf (1966) refer to unusual versions of the Müller-Lyer figure shown in Fig. 7. It is difficult to see either of these figures in depth, yet the illusion is maintained. On the other hand, Humphrey & Morgan (1965) give a figure in which they claim that there is clear perspective, yet no illusion (Fig. 8).

The present writer has no alternative theory to offer. It seems that Hotopf (1966) is to a large extent correct in his claim that the visual illusions are multiply determined. A mechanism can be suggested for the distortions, but the explanation of the illusions is probably different.

My thanks are due to H. L. Wagner and to Prof. G. Westby for helpful discussion and suggestions.

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AGE AND LEARNING IN QUAIL

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The relation between age and learning capacity was investigated in two species of quail, and in two different experimental situations. Detailed curves were obtained by testing a large number of different age-groups between hatching and maturity. The results show age changes which are not monotonic in character, but suggest the interplay of a number of different factors.

1. INTRODUCTION

Early work on the way in which the learning ability of an animal changes with age (for summaries of which see Denenberg & Kline, 1958; Vince, 1960*a*) gave results which appear inconsistent; that is, older animals sometimes appear better than younger ones, sometimes worse. This is not necessarily inconsistent if performance does not change monotonically with age, but improves over some age ranges and/or deteriorates over others. Moreover, the way in which performance changes will depend on the actual task.

Our aim in the present series of experiments was to obtain curves as detailed as possible, by testing at many different ages between hatching and maturity; it was also to check the results, first, in a related species with markedly different rate of growth and, secondly, in a different type of learning task. In the second task two groups of birds were tested, which were reared under different conditions. The animals used were precocial birds because these can run about and feed themselves, and can therefore be tested, from a few hours after hatching. The two species were the bobwhite quail (*Colinus virginianus*), and the Chinese painted quail (*Excalfactoria chinensis*). In both experiments each individual bird was tested only once.

2. SPECIES CHARACTERISTICS, PRE-TESTING AND TESTING ENVIRONMENTS

The quail chicks were hatched in an incubator and subsequently were kept indoors with heat available at about 90° F (32·2° C) until their plumage was well grown. Heat was always provided by an infra-red lamp and food and water (turkey starter mash, a few live insects and green food or, under test conditions, mash only) were always available. The bobwhite chicks weigh about 5–9 g at hatching and grow to their full size and weight (about 200 g) in about 14 weeks. The painted quail weigh approximately 3–5 g at hatching and reach their adult weight (45–60 g) and begin to lay eggs in about 7–8 weeks. Bobwhites were reared indoors in cages measuring 5 × 2 × 3 ft. with hardboard back and sides, a cloth top, a front of ½ in. wire mesh and a solid floor. The painted quail can get through the ½ in. wire mesh at first, therefore they were reared in cages with solid sides and a wire top up to 3 weeks of age, when they were transferred to cages measuring 3 × 1½ × 1½ ft. and with wire fronts. The bobwhites in Expt. 2 which were reared under more varied conditions were kept out of doors on grass, in a 6 ft. wire run. They had a solid coop which was heated to 90°F when the chicks were very young. Birds which had reached sexual maturity were caged in pairs, to prevent damage from aggression, but all birds were kept socially, except when they were tested. In Expt. 1, 52 bobwhites and 53 painted quail were tested, and in Expt. 2, 91 indoor-reared and 50 outdoor-reared bobwhites, each bird at only one age.

In both experiments all birds were tested singly, each one in a cage measuring 2½ × 1 ft. with solid back and sides, a cloth top and wire front. Pairs of cages were arranged end to end in three tiers and in the case of very young birds infra-red brooder lamps were placed between the tiers. As quail tend to run about making 'distress' calls when caged singly the end of each cage nearest the centre was removed and replaced by net, or in some cases by a mirror so that an experimental bird could stand close to another bird or its own reflexion.

3. EXPERIMENT 1

Method

In the first experiment birds were required to discriminate between a box with a white lid which was the positive, reinforced stimulus (given a peck on the lid the box opened, revealing food), and the same box, empty, with a grey lid, which was the negative unreinforced stimulus (the lid opened when pecked, revealing an empty box). In the 60 test trials most birds learned to avoid the empty box; the score used was the time taken by the bird to peck at the lid, and a good performance was therefore reflected in short positive and long negative latencies. The test situation is illustrated in Plate 1. The box used was 2¾ × 1½ in. in size and approximately ½ in. deep. It had a sliding lid and opaque sides. It was opened by the experimenter, by pulling on a cotton attached to the lid, the other end of the cotton passing through a hole in a screen which concealed the experimenter from the bird. Pecking latencies were timed with a stop-watch. The food used for reinforcement was a preferred food given at no other time during the test period (a mealworm, blowfly larva or pupa, or millet—there was no food which all birds would take equally readily). On every trial the box was placed about 9 in. from the central end of the cage, i.e. away from the end where net, mirror or brooder lamp were placed.

Table 1. *Schedule for testing*

Day 1. Birds placed in cages for testing
Day 2. Given first trials with preferred food in open box
Day 3. Training trials
Day 4. Training trials. Lid closed more and more
Day 5. Training trials
Day 6. Bird trained to peck at closed box
Day 7. No testing
Days 8–13. Six days testing in differentiation situation

Training and testing occupied 13 days, and were carried out according to the schedule in Table 1. The aim of training was to get the bird to peck at the white lid as soon as the box was placed in the cage. On early training trials the box was presented with the lid open and the food inside clearly visible to the bird. On subsequent trials the presentations were made with the lid more and more closed, and finally with the lid entirely closed and the bait invisible. The birds were trained to the criterion of four consecutive successful trials (pecking at the box within a few seconds of its being placed in the cage). The number of training trials was adjusted so that birds reached criterion on day 6.

On days 8–13 birds were given 6 test days, with five positive (white lid, reinforced by food) and five negative (grey lid, box empty) trials on each day in random order. The box was always opened when the bird pecked at the lid, was removed on positive trials when the food had been eaten, and on the negative trials, and on positive trials if the bird failed to respond, it was removed after 100 sec.

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Results

Results are given in Figs. 1*a* (bobwhite quail) and 2*a* (painted quail), where performance is plotted against age and a single score is given for each bird tested. Age is always assessed as the number of days or weeks from the time of hatching to the first day of each test (day 8 in Table 1). The single score was obtained by subtracting

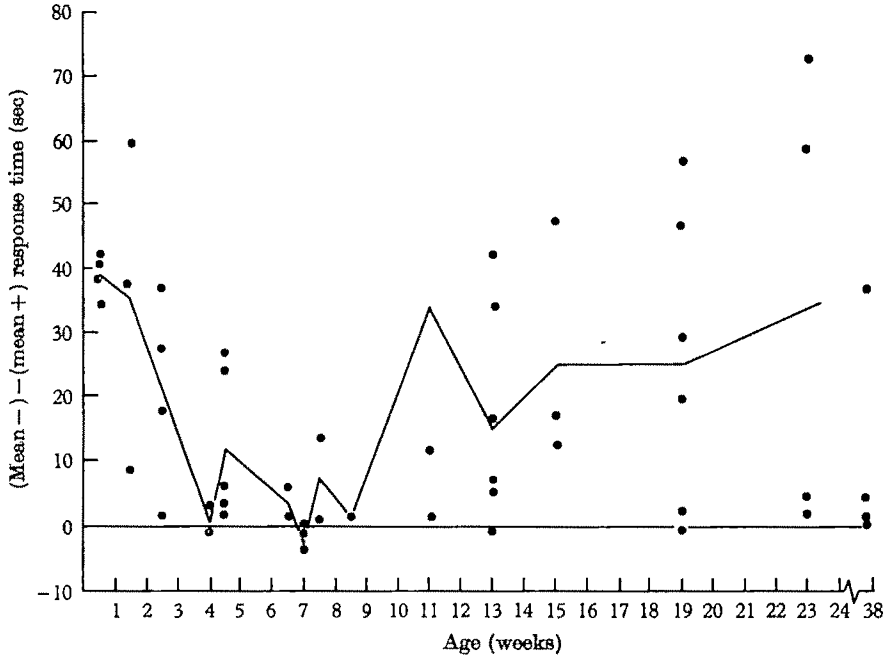

Fig. 1*a*. Expt. 1: Effect of age on performance; bobwhite quail.

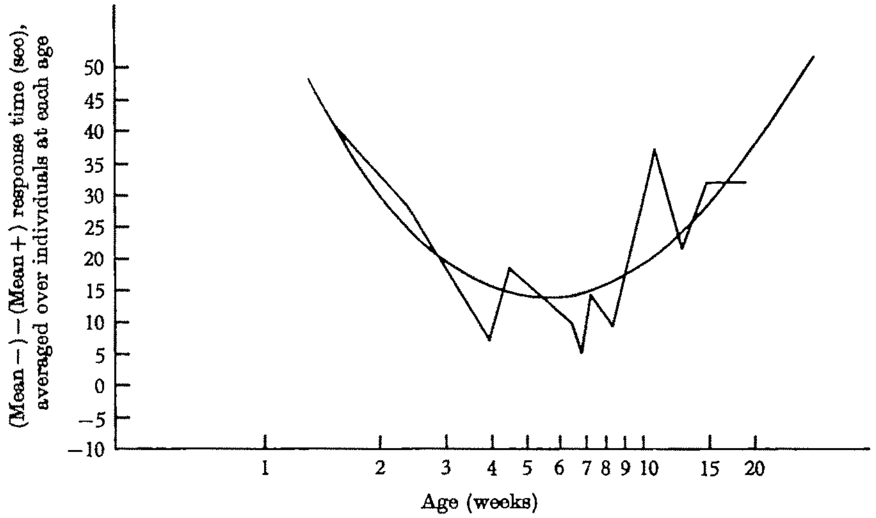

Fig. 1*b*. Expt. 1: Quadratic curve for bobwhites.

the average positive latency obtained from each individual (mean of 30 trials) from the average negative latency. Only negative trials after the first on which the bird responded to the grey lid by pecking at it were considered in obtaining this mean, as some birds avoided the unfamiliar grey lid on the first one or two trials and these long negative latencies (which give no information about the learned discrimination) would have biased the results unduly. Latencies on positive trials were considered because some birds were slower than others in responding on all trials, and here again, the longer negative scores alone would have biased the results and do not reflect a learned discrimination between positive and negative stimuli. The means for different age-groups are joined by a continuous line.

In Fig. 1*a* the bobwhites show a fair capacity to make the discrimination when recently hatched, and very poor discrimination between 4 and 8+ weeks of age. From 10 weeks their performance again improves slowly, although the oldest group tested gave only a moderate result. The mean for painted quail shows a similar tendency: in their case there was some improvement after the earliest age of testing followed by a gradual deterioration to a very low level between 4½ and 6 weeks. The means then show improvement with age, earlier than the bobwhites, followed again by some deterioration. In both species and in most age-groups there was considerable variation between individuals.

The formal statistical analysis was restricted to work on the individual scores for discrimination; each score was the difference between the mean negative response time (usually 29 observations per bird) and the mean positive response time (30 observations per bird). We chose to do this because such a score might reasonably be assumed to be distributed normally, whereas the scores for positive or negative response times would be considerably influenced by the upper limit of 100 sec on the observation time, as well as by the fact that they could never be negative.

An analysis of variance was done for each species to see whether there was any evidence that behaviour changed with age. The results of these are shown in Table 2.

Table 2. *Analysis of variance*

Source	S.S.	D.F.	M.S.	V.R.
		Bobwhite quail		
Between age-groups	7,703·5	14	550	1 31 not sig.
Within age-groups	15,535·0	37	419	—
Total	23,238·5	51	—	—
Mean	20,724·1	1	20,724·1	49·4 sig. at 0·1 %
		Chinese painted quail		
Between age-groups	23,033·3	13	1,770	8·1 sig. at 0·1 %
Within age-groups	8,534·7	39	219	—
Total	31,568·0	52	—	—
Mean	56,543·6	1	56,543·6	258 sig. at 0·1 %

The sum of squares due to the overall means are introduced into this table so as to test whether there is, in fact, any discrimination. Both means differ significantly from zero. The actual mean values are 19·9 sec for bobwhite, 32·6 sec for Chinese painted quail. (This corresponds to a mean positive response time of 9·1 sec and a mean negative response time of 29·0 for bobwhite; similar figures for painted quail are 17·8 and 50·4.) There is a significant difference between age-groups for the Chinese painted quail but not for the bobwhite.

However, inspection of Fig. 1*a* suggests that the bobwhite quail gradually deteriorate in performance for the first 7 or 8 weeks and then improve, and that this might

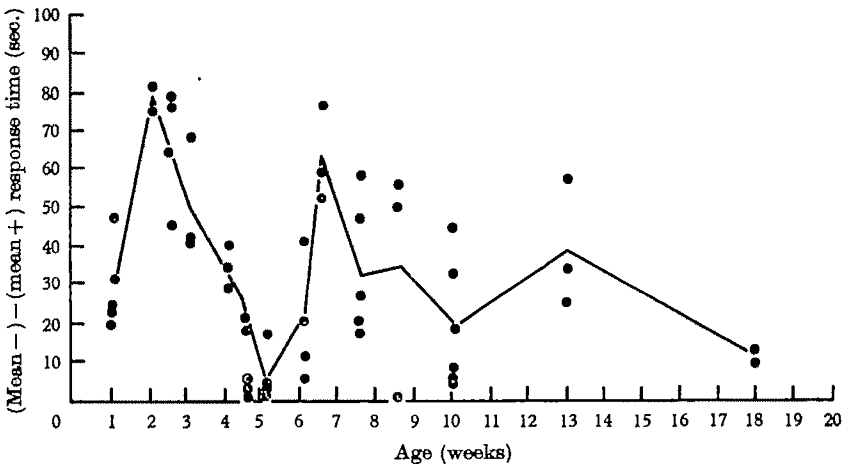

Fig. 2*a*. Expt. 1: Effect of age on performance; Chinese painted quail.

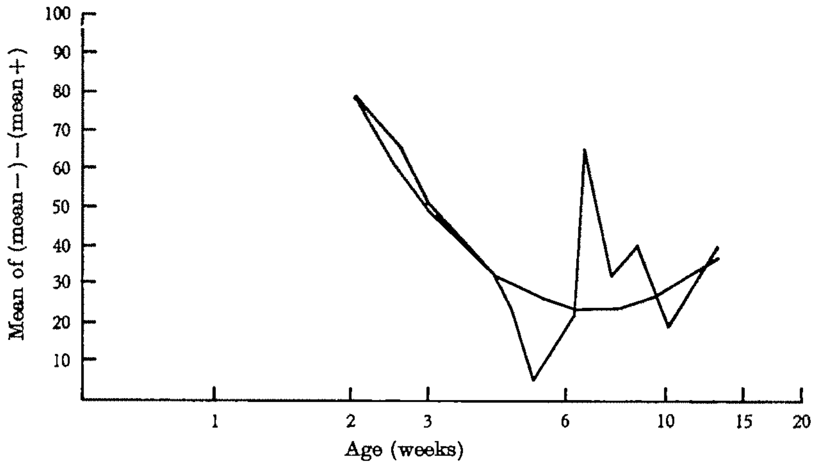

Fig. 2*b*. Expt. 1: Quadratic curve for painted quail.

be detected if a quadratic curve were fitted to the data. Since our aim was to fit the simplest possible curve, and also to estimate the age at which performance was worst, we decided to discard some of the data before fitting the curve. We omitted the birds aged about 1 week or less, and also those more than 21 weeks (this being $1\frac{1}{2}$ times the age to reach full weight). Also we used $x =$ log (twice age) instead of the actual age as it gave a more symmetrical graph (Fig. 1*b*). The best fitting curve of the form

$$y = a + bx + cx^2$$

(where y is the score) was

$$y-7\cdot4 = 83\cdot4(x-0\cdot76)^2$$

so that the score is a minimum at the age of about 5¾ weeks, the expected mean score being then about 7½ sec.

The analysis of variance for the 40 birds included in the age range considered is given in Table 3. The significance of the quadratic term makes it reasonably certain that there is an age effect, and that it was not detected as a difference between age-groups because the decline and increase are pretty symmetrical about the minimum.

Table 3. *Analysis of variance for quadratic curve*

Source	S.S.	D.F.	M.S.	V.R.
		Bobwhite quail		
Regression: Linear term	1·2	1	1·2	< 1
Quadratic term	3,208·4	1	3,208·4	8·65 sig. at 1 %
Variation of means about regression	1,514·8	9	166·1	< 1
Between groups	4,724·4	11	429·5	1·16 not sig.
Within groups	10,398·6	28	371·4	—
Total	15,123·0	39	—	—
		Chinese painted quail		
Regression: Linear term	4,715·0	1	4,715·0	20·4 sig. at 0·1 %
Quadratic term	4,163·9	1	4,163·9	18·0 sig. at 0·1 %
Variation of means about regression	13,034·2	9	1,448·3	6·26 sig. at 0·1 %
Between groups	21,913·1	11	1,992·10	8·7 sig. at 0·1 %
Within groups	7,859·5	34	231·2	—
Total	29,772·6	45	—	—

A similar method was used on the painted quail data, omitting birds of 1 week and also those of 14 weeks or more in age (this is again roughly 1½ times age of full growth). It is clear from the graph (Fig. 2*b*) that fitting a quadratic curve is likely to be less satisfactory in this species; in fact, although both the linear and quadratic terms are significant, so also is the residual variation about the fitted line. The equation of the best fitting quadratic is

$$y = 264\cdot1-422\cdot0x+185\cdot4x^2,$$

which has a minimum value of $y = 24\cdot0$ at $x = 1\cdot138$; that is, the score has an expected minimum value of 24 sec at about 6¾ weeks.

It seems clear that there is a deterioration in performance somewhere between the ages of 5 and 7 weeks for both groups of birds; it might be profitable to investigate this age range more intensively.

4. EXPERIMENT 2

Method

Expt. 2 was designed to dispense with the initial training period, and thus to include newly hatched chicks. Chicks will drink water very soon after hatching and the test objects were fluids, the birds being given a choice between tap water, and a solution of sucrose octa-acetate, a harmless bitter substance which bobwhite quail are known to avoid in a situation where taste can be associated with colour (Brindley, 1965). More recently Brindley & Prior (1968) have shown that this capacity to discriminate on the basis of taste does not change with age, again when taste can be associated with colour. In this experiment, therefore, a successful subject was one which avoided the bitter substance, and drank water.

Two groups of bobwhite quail were tested: a group reared indoors and a smaller group reared on grass, out of doors (see Section 2 above).

Birds to be tested were housed singly in the same cages as before. Two 'Flomatic' drinking fountains, both with pale green plastic troughs, were clipped on the outside of each cage at the end farthest from the lamp, so that the troughs protruded into the cage through circular holes (one can be seen in Plate 1). Birds were put into the testing cages on the day before the test began and at this time water was given in both drinkers. During the test period water was given in one drinker, and SOA in the other, and the drinkers were weighed and their positions reversed every 24 hr. As SOA is very insoluble the standard method of preparation was to add 5 g SOA to 400 cm^3 of tap water at room temperature. This was whisked for 10 min and then filtered. On the first test day one drinker was filled with the solution of SOA, weighed and placed with the trough projecting through the left-hand hole; a weighed drinker containing tap water was placed to the right. At the same time on the next day the drinkers were weighed and their positions reversed. The fluids were not normally renewed in the course of the test. Four readings in all were taken and for a comparison of results the amount of SOA drunk was expressed as a percentage of total fluid intake.

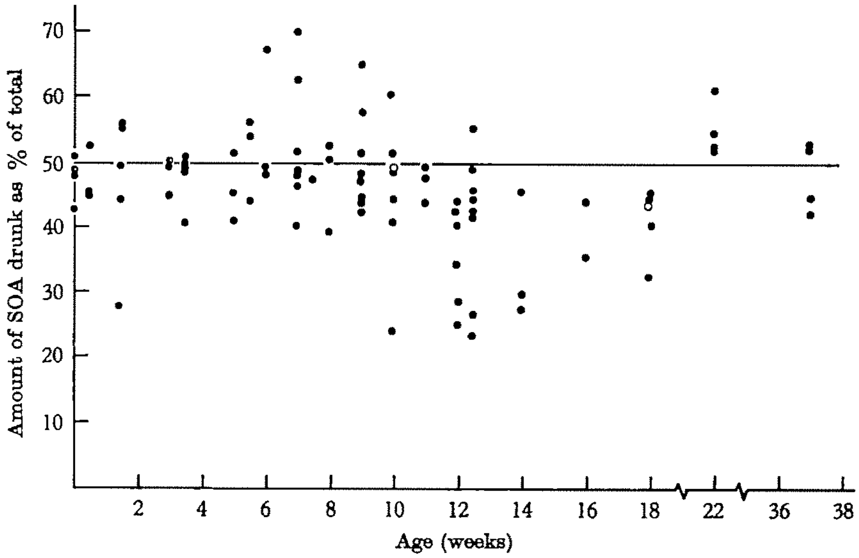

Fig. 3. Expt. 2: Effect of age on performance; indoor-reared birds.

Results

Results are given in Figs. 3 and 4 for indoor and outdoor birds respectively. These show that there was no evidence of discrimination before the age of 6 weeks. The birds showed first some preference for, and later some rejection of, the bitter fluid. In older birds the response to, or capacity to avoid, the bitter substance fell off, preference or rejection then being less marked.

The difficulty about showing that there was discrimination from these data lies in the fact that apparently some birds discriminate and reject, others discriminate and accept. Therefore the mean score, over birds of the same age, for the percentage of SOA drunk could be zero either because about an equal number of them were rejectors or acceptors or because none of them could discriminate.

We therefore measured the mean effect for each individual bird in terms of its own variability, as follows: call its score on the four test days d_1, d_2, d_3, d_4. Then the combined scores

$$x = d_1+d_2-d_3-d_4,$$
$$y = d_1-d_2-d_3+d_4,$$

should each have mean zero; that is, deviations from mean zero can be regarded as chance fluctuations. These combinations were chosen because the SOA was presented alternately on the left and right sides, and there might have been a side preference; in x and y, side preferences will balance out. (Any learning effect has been neglected.) Therefore $s^2 = \frac{1}{2}(x^2+y^2)$ estimates the variance of a combination of four scores. We can therefore assume that $(d_1+d_2+d_3+d_4-200)/s$ is distributed as Student's t, on two D.F., if there is really no preference; 200 is the total score over 4 days if 50 % is chosen on each day.

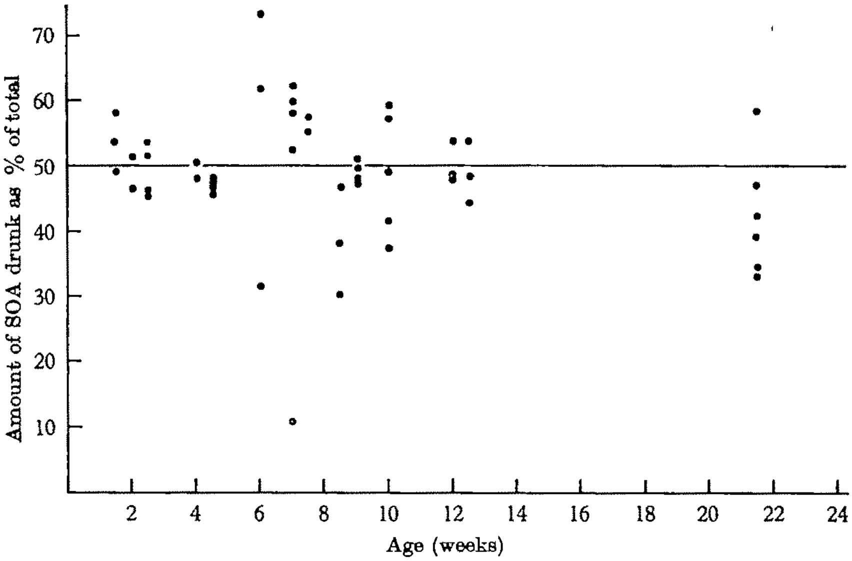

Fig. 4. Expt 2: Effect of age on performance; outdoor-reared birds.

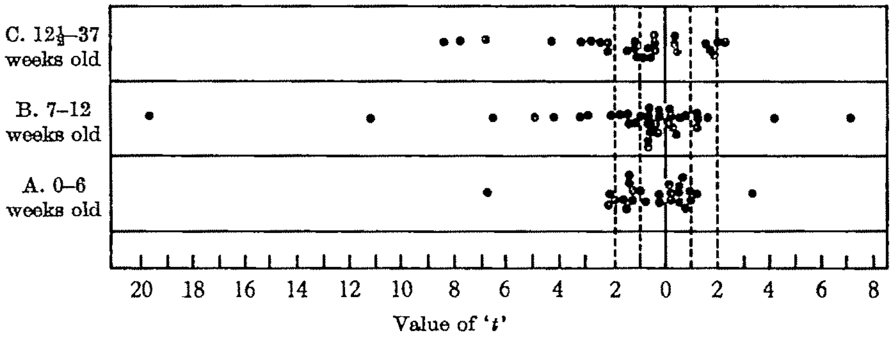

Fig. 5. Expt. 2: t-values for different individuals in three age-groups.

From the tabulated t-distribution we could find values $t_{10}, t_{30}, t_{50}, t_{70}, t_{90}$ which are such that we would expect 20 % of the birds to have t-values between t_{10} and t_{30}, and so on (t_{50} corresponds to the value 0). These chosen values are shown as vertical lines in Fig. 5 and the *t*-values obtained from the birds are plotted in relation to these, for three different age ranges.

Although many birds cluster near the point 0, and in the youngest group there are only five birds outside the t_{10} and t_{90} lines (the expected number being about six) in the mid-age group there are ten in the two end-bands, when seven are expected, and in the oldest group there are eleven, when about five would be expected; there are also some very large negative deviations. Therefore some birds in the two older groups can discriminate, probably none in the youngest group can.

DISCUSSION

Age changes observed in animal experiments have been considered by different people in different ways: e.g. Stone (1929*a*, *b*) interpreted them in purely motivational terms. Vince (1960*a*, *b*) considered them in relation to Pavlov's idea of internal inhibition strengthening gradually with age and experience, and Denenberg (1958, 1966) has discussed a number of hypotheses. In the present experiments an attempt has been made to find out the exact shape of the curve when a large number of points are taken between the times of hatching and maturity. In the first experiment described it was found that this curve is not monotonic in character, but the young birds show first a decline and then an improvement in their capacity to differentiate. In the second experiment the youngest birds appeared unable to avoid the bitter solution, then, at roughly the age when performance in Expt. 1 fell off, they began to show some discrimination (sometimes in an abnormal direction in the light of Brindley's findings), rejection being most marked at about the age when the birds in Expt. 1 were again improving.

In each experimental situation there are two groups which provide similar results, confirming each other (the two species in Expt. 1 and indoor- and outdoor-reared bobwhites in Expt. 2), but the two different situations give results which differ in a number of ways; what they have in common, in the main species, the bobwhite, is an improvement in the birds' performance at the age of about 7 weeks. This could represent an important stage in their development and might well repay detailed investigation. The time when this improvement begins is not so clearly marked in the painted quail.

In Expt. 1 there is a dip in the curve which precedes the improvement at 7 weeks; this occurs in both species. Such a dip has been reported before (Vince, 1960*a*) in wild-reared great tits tested over the first year of life, although here the numbers were very small. It has subsequently been confirmed with larger numbers in hand-reared great tits and will be discussed elsewhere (Vince, unpublished). In the great tit which has a short breeding season in May and June only, the dip could have been linked with increased motivation in the short winter days, but the quail can be induced to breed all the year round, and thus this factor was reduced in the present experiments. In mice, Denenberg has related a similar dip to sensory deprivation, a factor which would not apply to wild-reared tits, but could have affected performance in indoor-reared quail.

We have, therefore, complex age-changes which suggest that a number of different factors may come in at different times to affect performance.

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EFFECTS OF FOOD DEPRIVATION, DEPRIVATION EXPERIENCE AND SEX ON EXPLORATION IN RATS

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Following 0 or 48 hr current deprivation male and female rats were individually observed for 15 min in an exploration box consisting of a novel and a familiar half. Half of the subjects had gained deprivation experience by being allowed food for only 30 min a day for 10 consecutive days prior to testing. The remainder had been on an *ad lib.* diet for the same period. Current deprivation of 48 hr decreased locomotor activity (total number of units entered) only for deprivationally experienced rats, who showed more locomotion than naive animals when neither group was currently deprived. Also, current deprivation produced a higher frequency of exploratory activity for males but not for females. Males engaged in less exploration than females when neither sex was currently deprived. All rats showed an over-all preference for the novel half of the apparatus, but this preference was not affected by any of the three experimental variables. However, currently deprived rats entered the novel half sooner than non-deprived animals.

In 1953 Montgomery showed that 24 hr of food deprivation decreased locomotor activity in a Y maze by female rats. Thompson (1953) proposed that this depressing effect of deprivation on locomotion shown by Montgomery is characteristic only of females, because he found that male locomotor activity was increased by 48 hr of food deprivation, whereas the same period of deprivation decreased female locomotion. However, Zimbardo & Montgomery (1957), observing both male and female rats in a more complex square maze, confirmed Montgomery's finding, and, in addition, failed to show any sex difference.

In 1956 Fehrer noted that male rats, deprived of food for about 24 hr, explored a novel box, attached to a familiar home box in which each subject had been confined for 24 hr, sooner, more often and for longer periods of time than non-deprived rats. The food-deprived animals also showed less grooming behaviour, and remained motionless during fewer 5 sec behaviour-sampling intervals.

Hughes (1965) later confined male rats to one half of an exploration box with or without food for about 24 hr prior to testing. When given free access to both halves of the box all subjects showed a preference for remaining in the novel half during a 15 min period, but this preference did not differ between food-deprived and non-deprived rats. However, the food-deprived animals entered a greater total number of 8 in. square exploratory units.

In addition to locomotion, de Lorge & Bolles (1961) became interested in other kinds of exploratory activity and therefore tested male rats in an apparatus with windows designed to encourage visual and other forms of exploration. Using a behaviour-sampling technique they found that 24 and 48 hr of food deprivation increased scores on two of their measures of exploration (Investigation, Window peeking) and reduced grooming behaviour. However, locomotor activity was not affected. This study was followed by one in which male and female rats, maintained on either *ad lib.* feeding (deprivationally naive) or a cyclic deprivation schedule (deprivationally experienced), were observed in a Dashiell maze under conditions of 0 or 60 hr current food deprivation (Bolles & de Lorge, 1962*b*). Several measures of behaviour were recorded, including speed of entering the maze from the start-box, quantity and quality of locomotor activity, as well as the incidence of such obviously non-exploratory behaviour as freezing and grooming. Neither current deprivation nor sex had any significant effects, but prior deprivation experience outside the experimental situation did increase both the quantity and quality of locomotion, and decreased the amount of non-exploratory behaviour. These results indicated that in earlier studies in which cyclic feeding had been used prior to testing (e.g. Adlerstein & Fehrer, 1955; Glickman & Jensen, 1961), increased locomotor activity may have been a result of deprivation experience rather than the current food deprivation *per se*.

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It therefore seemed desirable to design an experiment incorporating most of the useful points raised by earlier workers with regard to the deprivation/exploration problem, namely, availability of both novel and familiar environments in a complex apparatus; records of several forms of behaviour in addition to total amount of locomotion; subjects of differing food deprivation experience and sex; test conditions of food deprivation and non-deprivation.

METHOD

Subjects

The subjects were thirty-two albino rats of the Wistar strain (sixteen male, sixteen female) approximately 150 days old. They were caged in pairs of the same sex in the experimental room.

Apparatus

The apparatus consisted of four wooden exploration boxes described in an earlier report (Hughes, 1965). Each box was 24 in. long, 16 in. wide and 8 in. high and consisted of two halves subdivided into three 8 in. square units by means of incomplete wooden walls 8 in. high and 2·75 in. wide. The gaps in these walls were 8 in. high and 2·5 in. wide. The two halves were separated by three guillotine slides which could be removed to allow access to a total of six units (see Fig. 1). The interior of each box was painted mid-grey and the floor was covered with sawdust. Each half of a box was covered by a hinged wire-gauze lid from which a water-dispenser nozzle was suspended in adjacent end units.

Procedure

Pre-test treatment. Four pairs of each sex were randomly assigned to a deprivationally experienced group which was allowed *ad lib.* food for only 30 min every 23½ hr over a 10-day period prior to testing, while the other four pairs were maintained on *ad lib.* food throughout this period (i.e. the naive group). The experimental design adopted required that both groups of rats be then randomly assigned in pairs to (*a*) another group deprived of all food for approximately 48 hr, or to (*b*) a group with *ad lib.* food for the same time. Prior to the 48 hr period, all subjects were allowed 12 hr *ad lib.* food so as to equalize deprivation levels before the introduction of the current deprivation conditions. The three variables, *deprivation experience* (*E*), *current deprivation* (*C*) and *sex* (*S*), constituted a 2 × 2 × 2 design.

Test treatment

Depending on the relevant current deprivation condition, each rat was confined to one half of a box for the last 12 hr of the 48 hr period with several food cubes (0 hr deprivation group) or

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The 45 possible card-pairs were then presented in optimal order (Ross, 1939), the order being reversed for alternate subjects with the sex ratio of the sample maintained within each sequence. Immediately following presentation of the 45th pair, the cards were re-presented to the subject in the same order, with the instructions to again indicate preferences between the members of each pair.

The second session, held one week later, involved administration of the CW scale.

It was predicted that inconsistency in preference behaviour would be directly related to variability in the cognitive-control dimension of breadth of categorization as measured by CW variance.

RESULTS AND DISCUSSION

The subjects obtained a mean CW variance of 2·27 with a range from 0·93 to 4·64. The mean preference inconsistency score was 6·46 with a range from 2 to 13. The correlation between CW variance and preference inconsistency was in the predicted direction and significant ($r = 0{\cdot}45$; d.f. 26; $P < 0{\cdot}01$, one-tailed).

Since it was conceivable that intelligence was negatively related to both CW variance and preference inconsistency, the effect of intelligence was held constant through partial correlation; the resulting coefficient was unchanged.

An alternative interpretation of the results might be concerned with motivational differences between subjects rather than the concept of variability in cognitive control *per se*. In an attempt to assess the effect of such differences, subjects were asked whether or not they 'would volunteer to participate in this or a similar experiment again'. Biserial correlation coefficients were then computed between this dichotomous index of motivation (46 per cent 'Yes'; 54 per cent 'No') and CW variance and preference inconsistency scores, yielding coefficients of 0·05 and −0·01, respectively, indicating negligible relationships.

A less direct approach to the problem of differential motivation was also attempted. It might, for example, be argued that low motivation would result in random responding or position responding to the alternatives on the CW scale. If such strategies resulted in larger than average CW variance, this, combined with indifferent and hence inconsistent preference behaviour,* would in turn produce a spurious relationship. As for random responding, several CW scales were completed with choices among the alternatives randomized; the resulting CW variances fell on both sides of the mean CW variance obtained by the subjects, with a slight majority of such randomly generated variances actually being smaller than the mean. The possibility of position-responding influencing the results was attacked by recording the frequency with which each subject responded to each CW scale alternative position, and assigning him a 'position-response score' equal to the frequency with which he responded to his most frequent position. However, when such tendencies to respond on the basis of position were controlled through partial correlation, the coefficient of correlation between CW variance and preference inconsistency was again unchanged. It would therefore seem that motivation was not a factor in the obtained relationship between CW variance and inconsistency in expressed aesthetic preferences.

In conclusion, a measure of variability in cognitive control was found to predict to the manifestation of inconsistent and otherwise unpredictable behaviour.

* It is conceivable that indifference *could* result in consistent-appearing preference behaviour if a subject merely chose either the 'left-hand picture' or the 'right-hand picture' throughout the preference task. However, the effect of such tendencies upon the relationship between CW variance and preference inconsistency as determined by partial correlation was negligible.

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VARIABILITY IN COGNITIVE CONTROL

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Inconsistency in preference behaviour was predicted on the basis of variability in cognitive control. Twenty-eight undergraduates performed an aesthetic preference task, and were subsequently assessed for intra-individual variability on the cognitive-control dimension of breadth of categorization. The results supported the hypothesis at better than the 0·01 level of significance by a one-tailed test.

The past decade has witnessed the growth of a sizeable body of theory and data concerning 'cognitive controls' or individual consistencies in processing stimulus input (e.g. Gardner, 1962). While considerable attention has been accorded inter-individual variation in these characterological response dispositions, the question of intra-individual variability in cognitive control has gone unexamined. Inasmuch as intra-individual variability is itself a fundamental and stable phenomenon (Fiske, 1961), variability in cognitive control should have certain predictable consequences. For instance, an individual characterized by a marked degree of fluctuation in cognitive control should find himself the victim of confused and contradictory information processing leading to inconsistent and 'unpredictable' behaviour. The present study was designed to test such an hypothesis.

METHOD

The dimension of cognitive control selected for study was that of breadth of categorization, i.e. the tendency to establish narrow or broad cognitive categories. Variability in breadth of categorization was assessed by means of the Category Width (CW) scale (Pettigrew, 1958). The CW scale consists of 20 items. Each item states an average value for a category (e.g. average number of lynchings per year in the United States). The subject is asked to choose one of four randomly ordered alternatives for each of (*a*) the upper limit of the category (e.g. the largest number of lynchings) and (*b*) the lower limit of the category (e.g. the smallest number of lynchings). The range between these two limits defines the width of the category. Values of 0, 1, 2, 3 are assigned to the four degrees of deviation from the stated average; thus a range of 0 indicates the narrowest category, and a range of 6 indicates the broadest category. The subject's variability in breadth of categorization is represented by the magnitude of his variance over the 20 items of the CW scale.

The behaviour to which variability in cognitive control was hypothesized to predict was aesthetic preference. This area of functioning was chosen because aesthetic preferences are meaningful to the subject while the criteria for preference are usually ambiguous to some degree, thereby generating a range of inconsistent preferences among subjects. The aesthetic preference task consisted of indicating preferences between the members of all possible pairs of ten Thematic Apperception Test (TAT) (Murray, 1943) cards selected for stylistic variety (cards 2, 8GF, 11, 12BG, 13B, 13G, 14, 17BM, 17GF, 19). The subject's inconsistency score was the number of within-pair reversals of preference made between a first and second presentation of the cards.

The subjects were 28 undergraduates, 15 males and 13 females. The study was conducted in two sessions. In the first session the subject was seated approximately 3 ft. away from a viewing stand which served to hold each pair of TAT cards, and instructed to 'choose which of the two pictures you like better'. A 5 sec exposure was considered sufficient for formation of a preference.

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enhanced recency effects.* One difficulty with this view is that should the enhanced recency effect following auditory stimulation itself have been due to an after-echo, then the fast auditory lists ought to have been better recalled, by all the children, than were the slow auditory lists. As a matter of fact, the 3/sec auditory presentation did give recall of the last items about 10 per cent better than that for the last items of the 1/sec auditory lists, for the 7- and 10-year-olds only. This difference was, however, insufficient to raise the total scores of 3/sec auditory lists noticeably above those for the 1/sec auditory lists. It is the writers' opinion that the close similarity between the three auditory curves of Fig. 1 may have resulted from the unusual method of enunciating the digits at presentation: as noted in the *Method* section, the experimenter dragged out the sounds of the slow lists while speaking them, in order that the auditory presentation would be more analogous to the visual in respect of the exposure time/inter-item interval relationship. This way of speaking the digits would ensure a tight inter-item linkage, or strong association between the successive sounds. Hence differences in the recall of the auditory sequences presented at different rates might be expected to be reduced because of highly similar degrees of associative strength between successive items at the three rates (cf. Wickelgren, 1967; Murray, 1967).

The fact that the span improved with age under all conditions (including auditory, and fast visual presentation) would suggest that an important component of this improvement would be an increasing proficiency in using language mediation for memorizing purposes. But possibly there also develops an increasing facility at retaining information available in image-form (the last items) while carrying out the task of retrieving items stored subvocally (the first items). This view is consistent with the recent discovery by Inglis *et al.* (1967) that children's recall of dichotically presented material increases with age, particularly on the message recalled last. However, it also seems likely that as children grow older they become faster at writing down their recall, leaving less time for forgetting. The general rise in memory span with age, under all conditions, would support this view. The fact that differential effects of mode and rate upon improvement with age were observed in the present experiment would indicate that the presence of this confounding factor need not invalidate our theorizing. But it is clear that more work should be devoted to an investigation of output interference as well as of input processing and retention processes in short-term memory in schoolchildren.

This work formed the basis of a thesis presented by B. Roberts, in partial fulfilment of the requirements for the Queen's University M.A. degree, under the supervision of the first author. Expenses were borne by N.R.C. Operating Grant APB-126 awarded to D.J.M. We are extremely grateful to Mrs D. Hall, the District Commissioner for Brownie groups in the Kingston region, for her gracious permission to visit the groups, and to the Brownie leaders for their co-operation. We also thank E. Griffiths and P. Owen for their assistance in making the films.

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* It was observed by the experimenters that some of the children actually attempted to say the digits out loud under the visual condition. Although this practice was prevented as far as possible (by instructions and by watching the group during the experiment) some surreptitious vocalization may well have been carried out. This observation is of course consistent with the general viewpoint that verbal coding enhances the recall of slow visual lists, but the reader should bear in mind that 'visual' here might really mean 'visual plus low vocalization' for some of our subjects.

most of the improvement with age in the recall of visual lists also resulted from an increase in recency effects. Thirdly, there were few differences, for any age group, between the recall of the fast as compared with the slow auditory sequences.

The first two of these findings can be postulated to be interlinked. If the advantage for auditory over visual presentation is indeed reflected in increased recency (as Murdock and Corballis found), and if it is also the case that by subvocalization one provides oneself with internal representatives associated with auditory as well as with motor/kinesthetic systems subserving memory storage, then the more one subvocalized, the more likely would it be that recall patterns characteristic of retrieval from auditory storage emerged. Thus as children grew older and became more proficient at subvocalizing the visual lists, so they would be expected to give

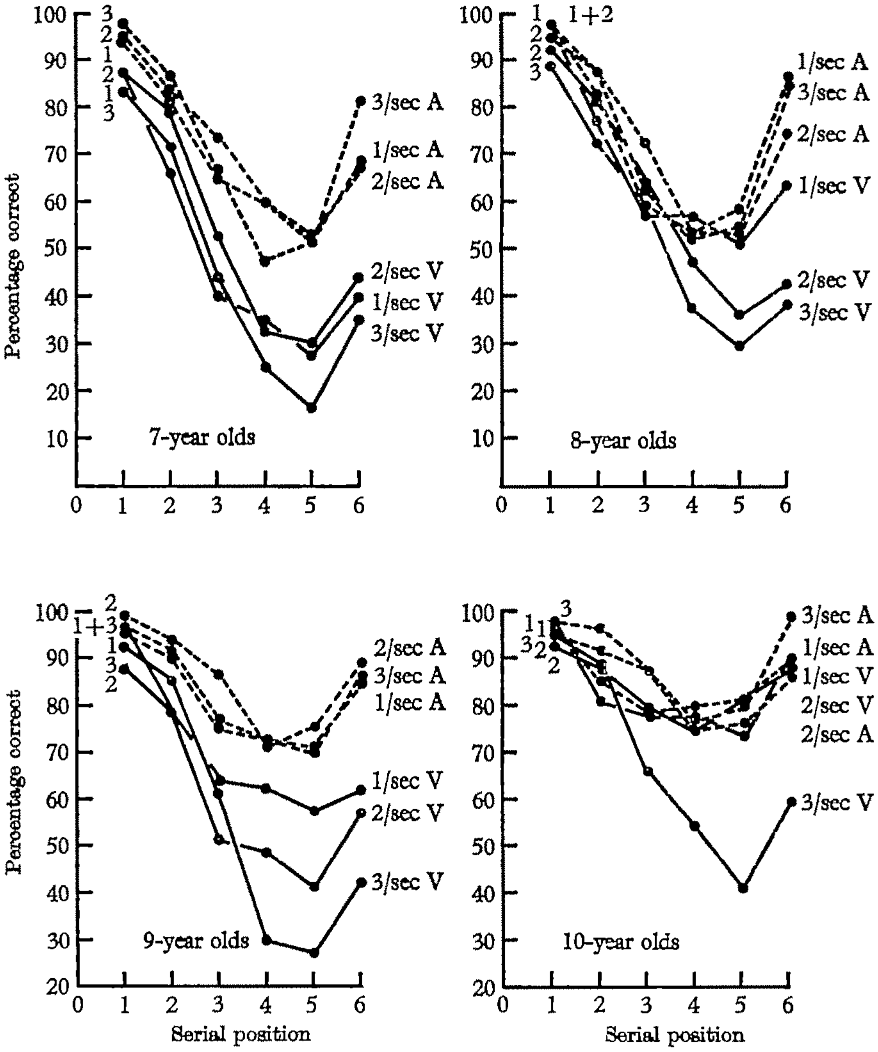

Fig. 2. Percentage recalled at each serial position as a function of age, mode and rate of presentation. *A* = auditory; *V* = visual.

× mode interaction was also significant ($F = 3{\cdot}12$, d.f. 6, 1200), and, as might be gathered from Fig. 2, so was the interaction rate × mode × serial position ($F = 2{\cdot}68$, d.f. 10, 2100). Turning to individual comparisons: that the total amount recalled by the 7-year-olds following auditory presentation was significantly greater than that recalled following visual presentation was indicated by the results of Scheffé's test applied to those data ($P < 0{\cdot}01$). Moreover, the difference between the recall of the slowest and fastest visual lists, for the 7-year-olds, was not significant, either by Scheffé's test or by Tukey's less conservative test (*a*) (see Winer, 1962, p. 77 ff.). But for the 10-year-olds the difference between the recall of the slowest and the fastest visual lists was significant by Tukey's test ($P < 0{\cdot}05$). This is consistent with the notion that the greater skill shown by these subjects at reading had indeed raised their recall of slow visual lists over that for the fast visual lists, as compared with the corresponding data for the 7-year-olds.

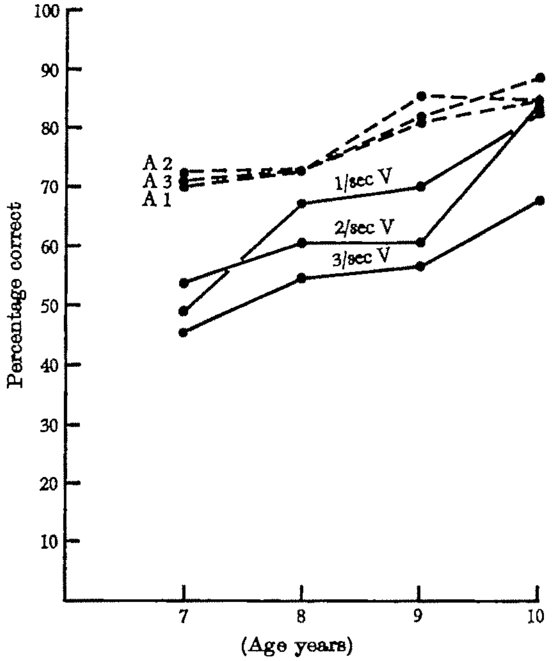

Fig. 1. Percentage recalled as a function of age, visual or auditory presentation, and presentation rate. *A*, auditory; *V*, visual.

Discussion

Our main prediction, that improvement of memory span with age should be most marked for the case of slow visual lists, was substantiated. Since this prediction was based on inferences made from an analysis of previous work on presentation modes and rates, the broad outlines of this analysis would therefore appear to be valid. However, three other findings emerged which should be taken account of in interpreting the described results. First, it was observed that most of the advantage for auditory over visual presentation was reflected in increased recency effects. Secondly, auditory presentation more analogous to the visual in respect of exposure time, the speaker uttered the digits slowly when recording the slow lists, and quickly when recording the fast lists.

Subjects. The children were all members of Brownie groups in the Kingston area, and were tested during one of their regular meetings. Six groups were visited, each of which contained a complement of 7- to 10-year-olds (and some 11-year-olds, but not enough to warrant inclusion of their data in this report). While the results from a total of 150 subjects are available, the complements of each age-group were rather unevenly divided between the six Brownie groups. In particular, only sixteen 10-year-olds were present, mainly concentrated in three of the groups. It was therefore decided to select only the data for sixteen subjects from each of the other age groups for analysis. (However, it may be stated that the general trends of the results as derived from all the subjects differed in no obviously important way from that obtained from the selected sample.) The sample was selected by taking, from Brownie groups one to six respectively, the following numbers in each of the four age-groups: 2, 2, 1, 3, 4 and 4. Apart from this constraint, selection was as random as the number of children in any age-group in any Brownie group permitted.

Procedure. Recall was written in answer books which contained a separate page for each list with a grid of six blank spaces on each page, one for each digit to be recalled. (In preliminary work, it had been found that the younger children would often write down only a few digits with no blank spaces, making the results impossible to mark for serial position effects. The use of a grid seemed to forestall this practice quite effectively.)

If a block of visual trials was first presented, three visual lists (the first at the slowest rate) were given for initial practice. When it seemed that all the children knew what to do, the block of visual test trials was presented. Then followed three practice trials under auditory conditions, one at each of the three rates, succeeded by the block of auditory test trials. For half the subjects, an auditory block of trials came first followed by the visual block, with the appropriate practice trials preceding each block.

Design. Six orders of presentation conditions were used, one for each Brownie group. Calling visual presentation V and auditory A, with rates of 1, 2 and 3 digits/sec, these orders were: V(1 2 3) A(1 2 3); V(2 3 1) A(2 3 1); V(3 1 2) A(3 1 2); A(1 2 3) V(1 2 3); A(2 3 1) V(2 3 1); A(3 1 2) V(3 1 2). This design allowed rates to be counterbalanced in order across groups. For each separate group, a separate film and tape were made. Since each age-group was represented in each Brownie group, rate and mode of presentation can be considered to be properly balanced across the age variable. As noted above, five lists were done under each condition, the lists themselves also being balanced across conditions.

Scoring. Digits were marked as correct only if they appeared in their correct position in the list.

RESULTS

Fig. 1 shows the percentage correct obtained, under each of the mode and rate combinations, by the sixteen subjects in each group. It can be seen that in general recall improved with age, and that the rate of improvement for recall of the slower visual lists was steeper than that for the fastest visual lists and on all the auditory lists. Not expected, in view of previous findings, was the consistency in the recall of the auditory lists presented at different rates. Fig. 2 shows the same data replotted so as to give the details of serial position curves.

An analysis of variance was carried out on the raw scores upon which were based the curves shown in Fig. 2. Significant effects (0·01 level) were found for age (F = 8·59, d.f. 3, 60), rate (F = 13·27, d.f. 2, 2100), mode of presentation (F = 334·82, d.f. 1, 2100), and serial position (F = 220·08, d.f. 5, 2100). The following two-way interactions were significant: age × mode (F = 14·12, d.f. 3, 2100), age × serial position (F = 6·14, d.f. 15, 2100, rate × mode (F = 20·01, d.f. = 2, 2100), rate × serial position (F = 4·67, d.f. 10, 2100), and mode × serial position (F = 16·09, d.f. 5, 2100). In accordance with the general outline of the predictions, the age × rate children). It would be expected then that if we took separate groups of subjects varying in reading skill and presented them with either visual or auditory lists at varying presentation rates, the poor readers would generally give better recall of the auditory as opposed to the visual sequences even at slow rates of presentation. On the other hand, the good readers would show better recall of auditory as opposed to visual sequences at fast rates, but not necessarily at the slow rates. At the slow rates the good readers would be able to encode the visual sequences verbally at presentation and hence raise their recall of slow lists to a level approximating that they obtained following slow auditory presentation. In order to test this prediction, we assumed that older children would be more fluent readers than would young children, and examined the hypothesis that the older children would give better recall of auditory as opposed to visual sequences at fast rates, but not at slow rates. For younger children, however, it might be predicted that recall of auditory sequences would be better than visual both at fast and at slow rates of presentation.

Although there are several, mainly early, studies investigating auditory *versus* visual presentation as they affect STM in schoolchildren, we have found none in which presentation rate was varied in addition to presentation mode. Conway (1909) reported that the memory span for auditory digits (1/sec) was superior to that for visual digits in 6-year-olds, with the difference steadily diminishing as age increased up to 14; and Hawkins (1897) found that the span for words (1 every 2 sec) was higher following auditory presentation for 8- to 15-year-olds, but higher following visual presentation for 15- to 20-year-olds. Also using words presented fairly slowly, Kirkpatrick (1894) found little difference between auditory and visual presentation in immediate recall. Other related studies (mainly concerned with learning rather than with immediate memory) are reviewed by Day & Beach (1950).

METHOD

General outline. Lists of six digits were presented either visually or auditorially, for immediate recall, at rates of 1, 2 or 3 digits/sec. The subjects were girls aged between 7 and 10.

Apparatus and materials. Thirty-six lists of six digits each were devised, with the constraints that no digits recurred in a list, there were no occurrences of forward or reversed successive digits (e.g. 45 or 54), and each digit appeared equally often in each of the six serial positions and therefore equally often over the whole series of lists. Six of these lists were used for initial practice, and the remaining 30 were divided into six sets of five lists each. The order of the lists presented was the same for all groups of subjects, but each set of five was done under a different experimental condition for each group (see *Design*, below).

For visual presentation, the lists were made up into films (8 mm) by photographing printed letters for a prearranged number of frames on the film. The projector was then set to run at a speed which allowed the digits to be presented at the appropriate rates. By this method, the digit to be memorized was actually visible to the subject for the whole duration of the interval between successive onsets of new digits. Four seconds before the onset of the first digit of a new list, there was a warning black 'flash' on the screen, and the experimenter also said 'ready'. Blank intervals of 30 sec elapsed between lists—this had been found, in preliminary trials, to be ample time to allow even the 7-year-olds to recall as much as they could of the preceding list. Intervals of 60 sec elapsed between successive sets of five lists, time enough for the experimenter to warn the subjects of the next speed to expect.

For auditory presentation, the lists were tape-recorded in a woman's voice, reading in time with a metronome (inaudible on the tape) at a rate of 1, 2 or 3 digits/sec. As in the case of the visual sequences, a warning 'ready' was spoken 4 sec before each new list, and 30 sec was allowed for recall following each list (60 sec between sets of five lists). In order to make the

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VISUAL AND AUDITORY PRESENTATION, PRESENTATION RATE, AND SHORT-TERM MEMORY IN CHILDREN

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Lists of six digits were presented either auditorially or visually at rates of 1, 2 or 3 digits/sec to groups of 7-, 8-, 9- and 10-year-old girls. Immediate recall improved with age under all conditions, but the recall of visually presented slow lists improved with age at a steeper rate than did the recall of visually presented fast lists or of auditorially presented lists. The results are related to the findings of previous studies on STM as a function of presentation rate, and in particular to the hypothesis that when slow rates give superior recall as compared with fast, it is because subjects can more fluently read and rehearse the items at the time of perception.

Two variables which exercise important effects on immediate memory performance are presentation rate (see Aaronson, 1967, for a review) and the mode, visual or auditory, by which the material is presented (see Day & Beach, 1950, and Margrain, 1967, for reviews). These variables also interact in determining immediate recall. For example, Mackworth (1964) found, using a variety of materials, that when presentation was visual, recall improved as presentation rate decreased; but with auditory presentation, recall improved as presentation rate increased. The range of rates over which the latter improvement was noted was 2–4 items/sec, a range over which fluent reading or subvocalization of visually presented successive items becomes difficult. Other workers who have found auditory presentation to be superior to visual, within that range of presentation rates, include Broadbent (1956), Murray (1965), Laughery & Pinkus (1966) and Margrain (1967). At slower rates, however, 1 item/sec or less, the recall of visual as compared with auditory items becomes more nearly equal (Brener, 1940; Pollack, Johnson & Knaff, 1959; Norman, 1966); at very fast rates (> 4 items/sec), the difference also appears to break down (Norman, 1966). A probable explanation for this rather complicated picture is that, with slow visual or auditory presentation, rehearsal during presentation is fairly easy, and recall is essentially of items which have been coded into speech. But at rates at which it becomes difficult to subvocalize at presentation, i.e. two or more items/sec, the recall of visual lists suffers in comparison with that of auditory lists. Moreover, the recall of auditory sequences might be enhanced at these rates because there may be a kind of 'after-echo' lasting 3 or 4 sec, from which retrieval could be made, and which would be absent in the case of visual lists. Typical evidence for such an after-echo includes introspective data concerning recall strategies (Aaronson, 1967), differential efforts of practice on the recall of fast as compared with slow auditory lists (Mackworth, 1965), the fact that speeding up the list as it is presented aids the retrieval of auditory (Schonfield & Donaldson, 1966) but not of visual lists (Corballis & Loveless, 1967), and stronger recency effects in the recall of auditory as opposed to visual lists (Murdock, 1966; Corballis, 1966).

An obvious prediction which could be made on the strength of the above hypothesis would be that persons who were not very fluent at reading, such as young children, should find the recall of rapid visual lists particularly difficult, but that their recall of rapid auditory lists need not be too different from that of fluent readers (e.g. older that a good part of the magnification of the retinal image resulted in neither reduced distance nor increased size. It would be of interest to know how this relationship was affected by change of instrument and, in particular, what is the relationship between apparent distance and apparent size when the subject is looking through the wrong end of a low-power telescope.

I hope that someone with the necessary laboratory facilities will find it of interest to pursue these further inquiries.

CONCLUSIONS

What these experiments indicate is that the effect of looking through binoculars at the distances investigated is no phenomenal expansion of space in a lateral direction but a very considerable phenomenal contraction of space in the direction between the observer and the object. This is in line with what was reported by Helmholtz. One may express this by saying that the perceptual advantage of the use of binoculars (apart from showing greater detail in objects) is to make them appear nearer, not larger.

If this contraction of phenomenal space in the direction between observer and objects holds also for greater distances than those studied (and there is considerable observational evidence that it does), then the illusion of the slow apparent motion of an approaching (or receding) car seen through binoculars is explained. If the spatial contraction in this direction is, at the car's distance, of the same order as for those investigated, one would expect such a car travelling at 40 m.p.h. to appear through binoculars to be travelling at about 11–12 m.p.h. This would be a very marked phenomenal slowing.

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with Helmholtz that the effect of such magnification was in fact to make the object look nearer and not to look larger, although his explanation of this fact was somewhat different from that of Helmholtz.

Neither Helmholtz nor von Kries appears to have done any experiments attempting measurement of this effect. The statement that the object looks nearer but not larger seems to be based on common experience. Both were concerned to find an optical explanation of this fact of common experience.

In brief, this explanation depends on the fact that the cues determining size and distance, when looking at an object at a distance d through binoculars or telescope of magnification $\times a$, will not be identical with those provided by an object at distance d whose linear dimensions are a times greater than those of the original object. They will resemble more closely the system of size–distance cues from an object of the size of the original object but observed from a distance d/a, but they will not be identical with these. The matter is further complicated by the fact that these cues differences will depend not only on image magnification but also on such factors as the elimination of binocular cues when a telescope is used or by the change in effective interocular distance produced by prismatic binoculars. Moreover, the perceptual effect of any such instrument is not completely predictable from optical considerations since different size–distance cues will be differently affected by the magnifying system. If, as seems likely, different individuals depend to different extents on the various cues that can be used for determining the apparent sizes and distances of observed objects, one would expect individual differences in the measurements of apparent size and distance for different percipients. Such individual differences were strongly indicated in my experiments. It was not merely that different experimental subjects gave different measurements; this might be due simply to the measurements being of low reliability. What gave strong indication of the reality of individual differences was that independent measurements of results for non-identical but closely related tasks showed high intercorrelation. This was the case for tasks III and IV ($r = 0{\cdot}82$) and for tasks V and VI ($r = 0{\cdot}96$).

The experiments reported here suggest that the matter is more complicated than the non-experimental observations of Helmholtz and von Kries led them to suppose. It may indeed be questioned whether optical considerations of the kind considered by von Kries can do more than indicate possible characteristics of visual perception through optical instruments. The expectations aroused by these indications may be falsified by the complexity of the neurophysiological dynamics of the process of seeing and by the possibility of individual differences in this process.

A minor respect in which the results of these experiments come into conflict with what was reported by Helmholtz was with respect to the apparent size of the object looked at through binoculars. He said that the apparent size of the object remained unchanged; in my experiments it was found, in general, to look smaller. Further experiments should be done with magnifying systems of different power to find out over what range of magnification the apparent size of the object is reduced, and whether at some amounts of magnification it begins to look larger.

Finally, an explanation of these effects in purely optical terms would lead to the expectation that the increased nearness and increased size would be complementary. With the instrument I used, it appeared that they were not complementary, and menal distance. In task V the actual distance of the standard was (as in the other task) 7·14 m from the subject's eyes. Column V*a* shows the mean distance in metres at which the comparison object was seen, with naked eyes, as at the same distance as the standard looked at through binoculars. Column V*b* shows this as a fraction of the actual distance of the standard. If the phenomenal distance were decreased by the full amount that could result from the magnification of the retinal image, the values in V*b* would be centred round 0·125. It will be seen that, with one exception, all values in column V*b* are in excess of this amount, and that their average is about twice this. What is indicated is that vision through binoculars produces considerable decrease of apparent distance, but that, at the magnification investigated, this decrease of apparent distance is about half of what would be expected if the whole amount of retinal magnification were reflected in decrease of apparent distance.

Task VI was concerned with the effect of the binocular's increased inter-ocular distance on perception of distance. The measurements for task VI are exactly the same as those for task V except that the measurements for task VI are obtained with one object glass of the binoculars blocked out so that the effect of enhanced inter-ocular distance is eliminated. Here also there is no sign of an appreciable effect of the increase of retinal disparity by the enhanced inter-ocular distance produced by the binoculars; the means of columns V*b* and VI*b* are approximately the same. This factor does not seem, in these experiments, to affect either apparent size or apparent distance.

This result is surprising since it is contrary to what would be expected on optical grounds and to what is generally asserted in textbooks. It is, however, uncertain whether the common textbook statement (e.g. Woodworth, 1947) that the increased inter-ocular distance produced by field-glasses increases the relief of distant objects is an assertion based on optical expectations or on experimental measurements. The present experiments suggest that, if there is such an effect, it must be a very small one.

DISCUSSION

The main finding of these experiments, that the use of such a magnifying instrument as telescope or binoculars is to decrease the apparent distance of an observed object and not to increase its apparent size, was known to Helmholtz and is discussed by von Kries in his notes to the third edition of the *Physiological Optics* (Helmholtz, 1962). I am grateful to Professor Ross Day of Monash University for pointing this out to me. Helmholtz's own statement (p. 342) was that the result of looking at objects through parallel telescopes or binocular field glasses is that 'the angles subtended at the eye by all the parts of the picture will be uniformly enlarged...this is equivalent to bringing the object closer and reducing its depth-dimensions without altering the dimensions at right angles to the line of sight. Accordingly, objects seen through binocular glasses are made to appear nearer, neither smaller nor larger, but flattened as in low relief.'

This is a considerable over-simplification of the situation, since there are other cues to perceived size and distance than those provided by the angles subtended at the eye by different parts of the observed object. von Kries discusses the effects of changes produced by such a magnifying system on other size–distance cues. He agreed variable at this point of balance is shown in the first column of the Table (I*a*); the height of the constant was 13·7 cm. In the second column (I*b*) this is shown as ratio of variable size to constant size. In the absence of any tendency to constancy, entries in I*a* would be centred round 6·85, those in 1*b* round 0·5.

The results of task I were of no independent importance in this research; they were needed as a basis for comparison with the results of tasks III and IV which measured phenomenal size when the subject was looking through binoculars. They are used as denominators in the ratios shown in columns III*b* and IV*b*.

The third and fourth columns (II*a* and II*b*) exactly correspond to the first and second columns but were obtained with monocular vision. As has already been mentioned, there was no application of the results of this task to any problem within this research.

Task III was the measurement of the extent to which phenomenal size was affected by looking at the standard object through binoculars. Column III*a* shows the mean height to which the variable was set when the subject reported it as phenomenally equal to the standard as seen through binoculars. Column III*b* shows this amount as a ratio to the apparent size of the standard when observed with the naked eye (i.e. as shown in I*a*).

If the × 8 magnification of the retinal image resulted in an equal magnification of the apparent size, the entries in column III*b* would centre round 8·0; if there were no magnification of apparent size these values would be about unity. In fact, for all subjects except one, these values are less than unity, indicating a reduction in phenomenal size by about a fifth on the average, although the stimulus size was increased by a factor of eight. This result seemed to me to be surprising; it would have seemed less so if I had remembered Helmholtz's observations, which will be referred to later.

Some part of the failure of increase of stimulus size to be reflected in phenomenal size may be due to reduction of perceptual context as a result of the restricted field of vision of the binoculars. As already reported, an attempt was made to estimate the magnitude of the effect of this factor on phenomenal size. Two subjects looked at the standard through a black box which cut out perceptual context without stimulus magnification. The small resulting decrease in phenomenal size (0·88 and 0·875) is obviously insufficient to account for a retinal magnification of × 8 not producing any increase in phenomenal size, although it may account for the small decrease in phenomenal size which generally results from looking at the object through binoculars.

Under task IV is shown a system of readings corresponding to those shown for task III, but obtained with a monocular magnifying system. If apparent size were a function of effective inter-ocular distance, the entries for task IV would be different from those for task III; if increased inter-ocular distance had the expected effect of decreasing apparent size, the figures for task IV would be greater than those for task III. This is not the case; change from a binocular system with enhanced inter-ocular distance makes no appreciable difference to apparent size.

While magnification of the retinal image did not increase the phenomenal size of the object looked at, the results of task V show that it did decrease its apparent distance. A possible explanation of the non-appearance of an increase of phenomenal size might be that the effect of retinal magnification was wholly absorbed by decrease of phenotask equivalent to that of task III except in the fact that in task IV the standard was looked at through a monocular magnifying system.

Task V was directed towards the measurement of the effect on phenomenal distance of vision through binoculars. The magnification of the retinal image which might be expected to produce increase of phenomenal size would also be expected to produce decrease of phenomenal distance. The variable object of changing size was no longer used in this and the next task, but a small object of the same size and shape as the standard was placed at variable measured distances from the subject's eyes. He was asked to look alternately at the standard through binoculars and at the variable distance object with naked eyes and to report whether the latter looked nearer, farther, or the same distance as the image seen through the binoculars. After the approximate distance of this point of balance had been discovered by preliminary measurements, the mean of a complete ascent and descent was taken as the phenomenal distance of the object as seen through binoculars.

Phenomenal distance might be expected to be more directly affected by change in effective inter-ocular distance than phenomenal size. Task VI was devised to test this possibility. It bears the same relation to task V as task IV does to task III. The measurements made were exactly the same as those of task V, except that the standard was looked at through a monocular magnifying system made by placing an opaque cap over one object glass of the binoculars. As in task IV, the subject made his comparison with the distance of the variably distanced object by looking at the latter with both eyes.

Table 1. *Summary of results*

	Task I		Task II		Task III	
Subject	(*a*) Height of variable (cm)	(*b*) Ratio V/C	(*a*) Height of variable (cm)	(*b*) Ratio V/C	(*a*) Height of variable (cm)	(*b*) Ratio III/I
1	12·0	0·875	8·8	0·64	10·6	0·88
2	12·0	0·875	9·9	0·72	6·75	0·56
3	11·75	0·86	11·25	0·82	7·75	0·66
4	15·5	1·13	16·9	1·23	8·6	0·555
5	10·0	0·73	9·1	0·665	11·6	1·16
6	13·75	1·00	12·0	0·875	12·25	0·89
7	13·5	0·985	11·25	0·82	11·1	0·82
Mean	12·64	0·92	11·31	0·825	9·81	0·79

	Task IV		Task V		Task VI	
Subject	(*a*) Height of variable (cm)	(*b*) Ratio IV/I	(*a*) Distance (m)	(*b*) Ratio D_V/D_C	(*a*) Distance (m)	(*b*) Ratio D_V/D_C
1	9·6	0·80	1·70	0·24	1·50	0·21
2	6·0	0·50	3·90	0·545	4·60	0·645
3	7·75	0·66	1·55	0·22	1·85	0·26
4	7·9	0·51	0·75	0·105	0·675	0·095
5	10·1	1·01	1·95	0·27	1·70	0·24
6	13·25	0·96	1·90	0·265	1·80	0·25
7	13·4	0·995	2·85	0·33	1·95	0·27
Mean	9·71	0·775	2·01	0·28	2·01	0·28

RESULTS

The numerical results of these six experimental tasks are shown in Table 1. Task I was concerned with the size of variable that was reported as having the same phenomenal size as the constant at twice the distance. The actual height of the size, it was explained to him that this changing appearance of size was what was meant by 'apparent size', and that I was going to change the size of the variable and ask whether its apparent size was greater or smaller than or equal to that of the constant. In all tasks the 'limiting' method was adopted. After a few preliminary observations to find the approximate adjustment for phenomenal equality, the measurement of the position for phenomenal equality was taken to be the mean position at which the subject had reported that constant and variable looked equal in a complete ascent and descent.

The experimental tasks

Each subject was given six experimental tasks. In task I the subject looked at the standard and the variable object with both eyes and under normal conditions of vision. The variable, which was at half the distance of the standard, was adjusted by the experimenter to the point at which the subject reported that it was of the same apparent size as the standard. Both objects were in a fully lighted laboratory with other objects visible at various distances. This is in the standard form for an experiment measuring phenomenal size constancy; its primary object, however, was not to discover each subject's index of size constancy but to obtain a measurement of the apparent size of the constant for each subject in normal vision in order that this might be used as a standard of comparison with the apparent size of the same object when viewed through binoculars.

Task II was the same as task I except that one eye only was used in looking at both standard and variable. This is a familiar form of experiment which can be used to show the effect on phenomenal size constancy of a reduction of distance cues. It had the usual result of making the apparent relative sizes of the test objects nearer to their actual relative sizes. The results of this task have not, so far, found any application to the present problem, so no more will be said about them.

Task III was more important. Its object was to find out how far the magnification (8 diameters) of the retinal image by the binoculars resulted in increased phenomenal size. Each subject looked at the standard through binoculars and at the variable with naked eyes, alternating between them as often as he wished before making his report on their relative sizes. As in tasks I and II, what was recorded was the mean height at which the variable had to be set in order that it should appear phenomenally equal to the standard.

Task III showed the unexpected result that, although the retinal image was magnified eight times, the phenomenal size of the standard when viewed through binoculars was not correspondingly increased but, for all subjects except one, was slightly smaller than when looked at with naked eyes. A possible explanation of this effect would be that it might result from a reduction of sensory information under this condition of viewing which necessarily cut out part of the perceptual context. The extent to which this factor of reduced information contributed to the effect could have been estimated by giving the subjects an additional task in which they looked at the constant through binoculars exactly similar to those used in task III but with the lenses removed so that there was the same reduction in perceptual context but without stimulus magnification. The problem had, however, not been foreseen and this additional task was not practicable under the conditions of the experiment. I did, however, try with two subjects the effect of getting them to look at the constant through a black box which allowed the object to be seen with both eyes while most of the perceptual context was cut out. Both subjects showed the expected decrease of phenomenal size (as compared with that under normal vision). The amount of the decrease was about the same for both subjects (0·88 and 0·875).

Stimulus magnification and reduction of sensory information are not, however, the only factors that may affect phenomenal size and distance in vision through prismatic binoculars. There is also an increase of retinal disparity through the optical centres of the object lenses being farther apart than are the optical centres of the unassisted eyes. What might be expected to result from this increase in effective inter-ocular distance is a decrease of the apparent distance of the object with consequent decrease of its apparent size.

Task IV was designed to measure the effect on apparent size of this change in effective inter-ocular distance. It was carried out in the same way as task III, except that the inter-ocular distance factor was eliminated by reducing the binoculars to a monocular magnifying system (i.e. to a telescope) by covering one object glass with an opaque cap. The constant was looked at through this monocular system but the variable was looked at with both eyes so as to make the distance, one would expect this to be reduced to one-eighth; if the apparent size only were affected, one would expect this to be increased to eight times in its linear dimension. If both apparent distance and apparent size are affected, will these effects add up to the total effect of magnification of the retinal image, so that, for example, doubling of apparent size by × 8 binoculars would entail that apparent distance was reduced to one quarter?

While spending a term at the Psychological Department at Adelaide University, Professor Jeeves very kindly allowed me the use of his laboratory which was of suitable length for these experiments and I was able to carry them out and to obtain some preliminary answers to these questions.

METHOD

The experiments were carried out on seven subjects who looked through a pair of 'Zenith' × 8 binoculars. The standard was a yellow triangle of height 13·7 cm and base 10·8 cm which was fixed at a distance of 714 cm from the subject's eyes. The binoculars could, for all subjects, be sharply focused on the object at this distance. For experiments on apparent size (i.e. the first four tasks given the subjects), the variable was a yellow triangle of the same shape as the standard but at half the distance from the subject (at 357 cm). My first intention had been to measure apparent change in size by having the subject look at the constant through binoculars and to compare the apparent size of the image with that of a variable triangle looked at with the naked eyes at the same actual distance as that of the standard. I rejected this procedure (perhaps mistakenly) because I thought that the task would be made unnecessarily difficult for the subject if he made the comparison between the magnified and unmagnified images under conditions in which he could see directly the relative actual sizes of standard and variable. For this reason, I put the variable at half the distance of the constant. The degree of phenomenal magnification was then measured as the ratio of the adjustment of the variable for phenomenal equality with the constant, when this was viewed through binoculars, to the corresponding adjustment for phenomenal equality when the constant was viewed with naked eyes. In both cases the variable was looked at by naked-eye vision, so this ratio was a measure of how much larger (or smaller) the constant looked through binoculars than it did when looked at with the naked eye.

In all of these experiments, the object was to obtain a report from the subject as to the apparent size (or distance) of the object looked at, not to obtain from him a judgement as to its actual size. In some cases, for example in looking at the object through binoculars, this intention would be clear from the nature of the task since the subject would not suppose that the actual size of the previously observed constant would be changed by looking at it through binoculars. In other cases, as in the first two (naked-eye) comparisons, the intention must be made clear to the subject by the instructions given. These instructions therefore included explanation as to what was meant by 'apparent size'. A portable yellow triangle was first shown, of the same size and shape as the constant and at the same distance. It was pointed out to the subject that both objects were of the same size and presumably looked the same size. All subjects agreed that they did. The portable triangle was then brought nearer to the subject without any change of actual size, and he was asked whether it was beginning to look larger. All except one agreed that it did.

The remaining subject (D) reported that the object appeared to become smaller as it was brought towards him. This was consistent with the fact that in task I, when he was equating for phenomenal equality between the constant and the variable at half the distance, he required the variable to be actually larger than the constant. This is a condition I reported in 1938 as being found in two subjects out of over 200 that I had tested (Thouless, 1938). I then called it 'super-constancy'. There has been a considerable literature on 'over-constancy' since that time and it has been stated to be a not unusual condition (Carlson, 1962; Wohlwill, 1963). This, however, applies to the different situation of the subject being asked to make a judgement about the actual sizes of objects at different distances. Phenomenal over-constancy (i.e. over-constancy in the situation in which he makes a report on the relative apparent sizes of objects at different distances) seems to be a rare perceptual condition.

Whether the subject saw the approaching triangle as increasing or as decreasing in apparent

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APPARENT SIZE AND DISTANCE IN VISION THROUGH A MAGNIFYING SYSTEM

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An approaching car, seen through binoculars, appears to be travelling very slowly. Experiments with seven subjects on the phenomenal size and distance of a test object looked at through binoculars (× 8 diameters) showed that the general effect was (as earlier reported by Helmholtz) to decrease the apparent distance of the object without increase of its apparent size. This effect could be expressed as a contraction of phenomenal space in the direction between object and percipient. The result of such a contraction would be to produce the illusion of decreased velocity in this direction which is, in fact, observed.

The problem

About 35 years ago I did some experiments on the effect of distance from the observer on the phenomenal size of an object he was looking at (Thouless, 1931, 1932, 1938). One problem that presented itself then was how looking at the test object through binoculars or a telescope would affect apparent size and distance. At that time I did only some preliminary experiments on this topic, and I did not publish any account of these.

The problem has, however, been brought back to my mind by reflexion on a curious illusion connected with the observation of a moving object through binoculars. If a car is travelling almost directly towards or away from an observer looking at it through binoculars, the car seems to him to be travelling very slowly. This is not a mere effect of there being few cues to distance change when an object is moving along or nearly along the line joining it to the observer, since the effect does not occur if the object so moving is observed by means of naked-eye vision; it is specific to the situation that the observer is looking through a magnifying optical system.

An explanation that suggests itself is that the effect could be due to the optical system causing contraction of phenomenal distances in a direction passing through the observer, so that an object moving along that direction will appear to be travelling over a shorter distance when seen through a magnifying optical system than when looked at with the naked eyes. The primary effect of such a magnifying system is to increase the size of the retinal image of the object. This is a change in sensory information which may be cue to two kinds of perceptual change: to increase of apparent size of the seen object or to decrease of its apparent distance. Either or both of these perceptual consequences may follow from the magnification of the retinal image of the object.

To what extent each of these results occurs when stimulus size is increased is a problem for experiment. If decrease of phenomenal distance takes place to an appreciable extent, the approaching car illusion may be easily explained.

There is a further question as to whether these effects are complementary. If one were using × 8 binoculars and their only perceptual effect was to change the apparent

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Lying malingerer scale. For contrasts I and II $P > 0{\cdot}05$, so these null hypotheses were retained, and for contrasts III and IV $P < 0{\cdot}05$, so these null hypotheses were rejected. Thus Prediction 3 was confirmed.

Table 4. *Summary of the analysis of the data from the LM scale using the method of planned contrasts*

Source	$\hat{\psi}$	S.S.	d.f.	M.S.	F
(I) Ho: $\mu_{FS} = \mu_{FSW}$	0·2	0·02	1	0·02	0·01
(II) Ho: $\mu_{FSW} = \mu_{C}$	1·8	44·72	1	44·72	1·29
(III) Ho: $\mu_{C} = \mu_{FNW}$	14·6	2889·50	1	2889·50	83·10**
(IV) Ho: $\mu_{FNW} = \mu_{FN}$	14·2	2729·81	1	2729·81	78·51**
Error		4520·07	130	34·77	

** $P < 0{\cdot}01$.

DISCUSSION

Since Prediction 1 was confirmed it can be concluded that subjects not warned of the presence of lie scales, and asked to simulate a stable or a neurotic personality, obtain more extreme scores on that variable than subjects warned of the presence of lie scales. However, such knowledge does not prevent these subjects from obtaining scores significantly different from those of a control group.

Since Prediction 2 was confirmed it can be concluded that groups asked to simulate neuroticism do not differ either between themselves, or from a control group, to an extent greater than σ, on a scale designed to detect subjects simulating stability. However, groups asked to simulate stability do have scores on that scale higher than those of a control group. They also differ from one another, subjects in a group warned of the presence of the scale scoring lower than those in a group not so warned.

Since Prediction 3 was confirmed it can be concluded that groups asked to simulate stability do not differ either between themselves, or from a control group, to an extent greater than σ, on a scale designed to detect subjects simulating neuroticism. However, groups asked to simulate neuroticism do have scores on that scale higher than those of a control group. They also differ from one another, subjects in a group warned of the presence of the scale scoring lower than those in a group not so warned.

Thus even when subjects are aware that a lie scale is included in an inventory it seems that they will be unable successfully to simulate stability or neuroticism (when 'successfully' is defined as obtaining the desired score and evading detection). Whether they can successfully simulate when given more information about the nature of the items used in lie scales requires further investigation.

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has the greatest power of all available tests (Hays, 1964; Rodger, 1965). Since it was proposed to interpret non-rejection of some of the null hypotheses specified in Tables 2–4 it was desirable to set a Type II error-rate (β) for the false acceptance of these. The values $\alpha = \beta = 0{\cdot}05$ and $n = 27$ were set, and these values (Guenther, 1965) allow the detection of a difference between population means as large as or larger than σ (where σ is the unknown population standard deviation).

RESULTS

The means and standard deviations for the three scales are reported in Table 1, and a summary of the analysis in Tables 2–4.

Neuroticism scale. For contrasts I–IV $P < 0{\cdot}05$, so these null hypotheses were rejected, and thus Prediction 1 was confirmed.

Lying stable scale. For contrasts I and II $P < 0{\cdot}05$, so these null hypotheses were rejected, and for contrasts III and IV $P > 0{\cdot}05$, so these null hypotheses were retained. Thus Prediction 2 was confirmed.

Table 1. *Means and standard deviations of scores on the N, LS and LM scales for the five groups*

		Scale		
Group		N	LS	LM
FS	Mean	8·2	17·4	4·4
	S.D.	7·2	7·2	2·8
FSW	Mean	15·9	13·1	6·2
	S.D.	10·2	13·6	4·6
C	Mean	24·8	7·3	6·0
	S.D.	10·7	5·4	4·6
FNW	Mean	39·2	8·6	20·6
	S.D.	8·2	8·6	8·4
FN	Mean	45·6	7·7	34·9
	S.D.	3·8	8·0	6·9

Table 2. *Summary of the analysis of the data from the N scale using the method of planned contrasts*

Source	$\hat{\psi}$	S.S.	D.f.	M.S.	F
(I) Ho: $\mu_{FS} = \mu_{FSW}$	−7·6	785·93	1	785·93	10·67**
(II) Ho: $\mu_{FNW} = \mu_{C}$	−8·9	1076·56	1	1076·56	14·62**
(III) Ho: $\mu_{C} = \mu_{FSW}$	−14·4	2803·25	1	2803·25	38·07**
(IV) Ho: $\mu_{FNW} = \mu_{FN}$	−6·4	547·79	1	547·79	7·44**
Error		9,571·48	130	73·63	

** $P < 0{\cdot}01$.

Table 3. *Summary of the analysis of the data from the LS scale using the method of planned contrasts*

Source	$\hat{\psi}$	S.S.	D.f.	M.S.	F
(I) Ho: $\mu_{FS} = \mu_{FSW}$	−4·1	257·81	1	257·81	4·05*
(II) Ho: $\mu_{FSW} = \mu_{C}$	−7·1	449·45	1	449·45	7·06**
(III) Ho: $\mu_{C} = \mu_{FNW}$	−1·3	23·88	1	23·88	0·38
(IV) Ho: $\mu_{FNW} = \mu_{FN}$	0·9	10·69	1	10·69	0·17
Error		8273·63	130	63·64	

* $P < 0{\cdot}05$, ** $P < 0{\cdot}01$

and (2) of a lie scale (LM) designed to detect those malingering, when completing a version of the MPI. The inventory used included the 20 items of the Keehn (1962) malingerer scale.

METHOD

Procedure

There were five groups of subjects:

(1) *Faking stable* (FS). This group was given the following instructions: 'Imagine that you have applied for a job which you particularly want, and that your prospective employers have asked you to fill in this inventory so that they can select an applicant with a stable personality. Please answer the questions so that you will appear stable (i.e. if necessary give false information so that you will seem a stable, mature person).'

(2) *Faking stable and warned of the lie scale* (FSW). This group was given the FS instructions, followed by: 'You realize, of course, that the people who construct personality tests will be aware that you may try to give a false impression, and that they will therefore include questions designed to catch you. To avoid detection as a "faker" you should be reasonably subtle, and avoid giving the impression that you are just "too good to be true".'

(3) *Control group* (C). This group was given the following instructions: 'We are gathering data which will allow us to obtain average scores on this personality inventory for students in the University. Please answer the questions as accurately as you can.'

(4) *Faking neurotic and warned of the lie scale* (FNW). This group was given the FN instructions, followed by: 'You realize, of course, that the people who construct personality tests will be aware that you may try to give a false impression, and that they will therefore include questions designed to catch you. To avoid detection as a "faker" you should be reasonably subtle, and avoid giving the impression that you are just "too bad to be true".'

(5) *Faking neurotic* (FN). This group was given the following instructions: 'Imagine that you are about to be called up for military service and wish to avoid this if it is at all possible. You are aware that you will be rejected if you have a neurotic personality. Please answer the questions so that you will appear neurotic (i.e. if necessary give false information so that you will seem an unstable, neurotic person).'

The instructions were attached to the modified version of the MPI. When the subjects were seated, the person testing the group said: 'In a moment I will give each person a personality inventory, with a set of instructions attached to the front. I want you to answer the questions according to the instructions. Do not spend too long on any one question, and try to avoid using the "?" category.'

The inventories were scored for N, LS and LM, with a score of 2 assigned to a positive response and a score of 1 to a '?' response.

Subjects

The subjects were 135 students in an introductory course in psychology. Although they had, as part of their laboratory course, completed some personality inventories, they were naive with regard to test construction, and lie scales had not been discussed with them. The students in laboratory classes were randomly allocated to the five experimental groups, and were given the instructions and completed the inventories in separate rooms. There were approximately equal numbers of men and women in each group.

Design

For the N scale it was predicted that

(1) $\mu_{FS} < \mu_{FSW} < \mu_{C} < \mu_{FNW} < \mu_{FN}$.

For the LS scale it was predicted that

(2) $\mu_{FS} > \mu_{FSW} > \mu_{C} = \mu_{FNW} = \mu_{FN}$.

For the LM scale it was predicted that

(3) $\mu_{FS} = \mu_{FSW} = \mu_{C} < \mu_{FNW} < \mu_{FN}$.

When $a-1$ (where a is the number of groups) predictions of this type are made before the data are collected, the appropriate method of analysis is that of planned contrasts, because it

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SIMULATION OF STABLE AND NEUROTIC PERSONALITIES BY SUBJECTS WARNED OF THE PRESENCE OF LIE SCALES IN INVENTORIES

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A version of the MPI, which in addition to the usual lie scale (LS) included a lie scale to detect malingerers (LM), was administered to independent groups of subjects under five sets of instructions. Group FS was asked to simulate stability, Group FSW to simulate stability and warned of the lie scale, Group C was given the standard instructions, Group FNW was asked to simulate neuroticism and warned of the lie scale, and Group FN was asked to simulate neuroticism. It was concluded (1) that on the neuroticism scale $\mu_{\mathrm{FS}} < \mu_{\mathrm{FSW}} < \mu_{\mathrm{C}} < \mu_{\mathrm{FNW}} < \mu_{\mathrm{FN}}$, (2) that on the LS scale $\mu_{\mathrm{FS}} > \mu_{\mathrm{FSW}} > \mu_{\mathrm{C}} = \mu_{\mathrm{FNW}} = \mu_{\mathrm{FN}}$, and on the LM scale $\mu_{\mathrm{FS}} = \mu_{\mathrm{FSW}} = \mu_{\mathrm{C}} < \mu_{\mathrm{FNW}} < \mu_{\mathrm{FN}}$.

It has been shown that when given appropriate instructions relatively naive subjects can answer personality inventories so as to appear neurotic (Keehn, 1962; Stanley & Salas, 1965), extraverted, aggressive or extremely normal (Braun & Gomez, 1966; Gomez & Braun, 1967; Handler & Shrader, 1966; Radcliffe, 1966; Stanley & Salas, 1965). Lie scales are often included in personality inventories in order to detect subjects attempting to give a favourable impression of themselves, the most notable exception being the 16 PF and its derivatives, although it is clear that such a scale is required (Handler & Shrader, 1966; Radcliffe, 1966). None of these inventories, however, includes a scale designed to detect malingerers. In many situations such a scale would be redundant, but it could be useful when subjects wish to appear neurotic (e.g. conscripts attempting to avoid military service). Keehn (1962) developed such a 'malingerer' scale and has shown that, when it is included in the Maudsley Personality Inventory (MPI), subjects attempting to simulate neuroticism are detected. If it is intended to use the malingerer scale it is, of course, necessary to include it in the inventory when norms are established, especially since the scale is made up of items reflecting extreme neurosis or psychosis. These items might act as 'anchors' (Helson, 1959) and tend to increase the number of responses to the items designed to measure neuroticism.

Using the Eysenck Personality Inventory, Braun & Gomez have shown that subjects instructed to 'make a good impression' (1966, p. 388) or to 'appear an ideal candidate for a position as a salesman' (Gomez & Braun, 1967) have lower scores on the neuroticism (N) scale and higher scores on the lie (L) scale than subjects given standard instructions. They also found that subjects asked to simulate but warned of the presence of the lie scale obtained L scores between subjects given the standard and 'faking' instructions. It appears that no research of this type has been carried out to establish whether subjects warned of the presence of a malingerer scale obtain similar scores when simulating neuroticism.

The experiment reported here was designed to establish whether subjects can simulate stability (S) (defined as low scores on neuroticism) or neuroticism (N) when warned of the presence (1) of a lie scale (LS) designed to detect those faking stable,

Professor P. E. Vernon writes: As Gordon Allport's first student when he came to teach at Harvard in 1930, and as one who has benefited ever since from his friendship and wisdom, I would like to add my tribute to him as a psychologist and as a man. He was born in 1897 in Montezuma, Indiana, brought up in Cleveland, Ohio, and obtained his first degree and his doctorate at Harvard by 1922. Thereafter he travelled, studied and taught widely in Germany, England and Turkey before returning, first to Dartmouth College, New Hampshire, in 1926, and then to Harvard. Already in 1922 his Ph.D. revealed his interest in the understanding of persons as unique wholes, in addition to comparing them by assessments on 'common traits'. His work abroad strengthened his regard—so rare among American psychologists—for the contributions of British, and especially of German psychologists and philosophers such as Dilthey and Spranger, and led to the emphasis on 'Verstehen' and on the Self as the central entity in personality, which are so prominent in his well-known books on personality, religion, prejudice and other aspects of social psychology. His influence outside the United States has been correspondingly great, though he also attracted and inspired many generations of brilliant psychologists in America—Cantril, Sherif, Baldwin, Holt, Bruner, Doob, Lindzey, to name but a few. When he partially retired in 1963, he was overwhelmed by the tributes and by a bound volume of their articles presented by some 60 of these past students at a secretly arranged dinner in Washington. To many of us he seemed the sanest, most humane exponent of psychology in a country and a university dominated by behaviourist empiricism.

As a man he was kindliness personified; urbane, cultured, humorous and above all sincere. He and his wife Ada had a deep love for New England, and it was a delight to walk with them in New Hampshire, or to visit their flat in Cambridge or their cottages on the coast in Massachusetts and Maine. When in 1963 he was awarded the Gold Medal of the American Psychological Foundation, it was accompanied by the following scroll:

'To GORDON WILLARD ALLPORT, outstanding teacher and scholar. He has brought warmth, wit, humanistic knowledge, and rigorous inquiry to the study of human individuality and social process.'

In honouring him, the British Psychological Society honoured itself.

Professor Henri Tajfel writes: It is always difficult to write a few words about someone like Gordon Allport without, somehow or other, appearing to advertise the fact that 'I also knew him'. I did, and my clearest memory is of how stable his image remained for me from before to after I came to know him. It is difficult, even when one reads technical literature, not to form some idea of the person who writes. The crucial test comes when one finally meets the writer. Gordon Allport lived up to the idea one had of him from his writings. I met him first when I spent at Harvard the academic year 1958–9. He had no responsibility for my visit, and neither he nor Harvard were ever short of visitors. Despite this, he was not only invariably kind and hospitable, but also continually took the initiative to make one feel welcome and at prepositional and thereby stressed the consistency with which traits determine behaviour.

Now while the trait was regarded as '*really there*' and a definite feature of mental structure, he affirmed (*Personality*, p. 314) that it could 'be discovered only through an inference (or interpretation) made necessary by the demonstrable consistency of the separate observable acts of behaviour'. Again (p. 319), traits were conceived 'on the physiological side as' undoubtedly neural dispositions of a complex order—and 'may be expected to show motivational, inhibitory and selective effects upon specific courses of conduct'.

To date, despite the impressive beginnings by Hess and Brügger in inducing specific patterns of behaviour in cats by stimulating different areas in the hypothalamus and the equally interesting studies initiated by von Holst and Ursula von St Paul in determining the reactions of fowls to different objects by electrical stimulation of different areas of the hypothalamus and lower regions, the confirmation of the existence and operation of traits which are 'really there' is not available on neurophysiological evidence and any empirical support must come from statistical studies. In June 1965 Allport read a paper in Durham before the North-East of England Branch of the British Psychological Society entitled 'Traits Revisited'. He referred to the assertions made in 1931 in a paper entitled, 'What is a Trait of Personality', read before the Ninth International Congress at New Haven. The basic features of traits, which were to appear in later works, were essentially as outlined above, and there followed a review of several empirical studies involving different occupational and religious groups. There was also an analysis of *Letters from Jenny*—a collection of 172 very frank letters from a remarkable woman—edited by Allport and published in 1965. The evidence generally indicated that the concept of trait as developed by Allport was at very least, a worth-while working hypothesis and deserving of further investigation.

The paper was memorable in many other respects. Four interesting decades of the history of the subject were revived. Due and courteous allowance was made for criticisms and throughout there was the characteristic fluency, erudition and clarity. From time to time, there were signs of slight respiratory difficulty and it now appears that his fatal malady had already begun; but mercifully the occasion was a very happy one for Allport and his wife. In that week, Durham conferred upon him the honorary degree of Doctor of Letters and the date of the actual ceremony coincided with their fortieth wedding anniversary. In personal correspondence, the events of the week have been described as the peak of their happiness together and indeed it must be stated that it was a pleasure and a privilege to entertain them. Whole days could pass without any mention of his professional associations, for both were keenly interested in the history of architecture, painting, folk art, music and historical developments in furniture and porcelain. Some tact was necessary to prise him away from the illuminated manuscripts in the Monks' Dormitory so that arrangements could be kept. It was completely natural for him to be keenly interested in cultural achievements and always with a degree of tolerance and humility which disguised his power and discernment.

One remembers a very kindly and very scholarly American gentleman, a worthy successor in the tradition of William James.

there is none to find, May it not be that the "irreducible unlearned motives" of men are—excepting in early infancy—a scientific will of the wisp?'

Allport's solution, which he kept under constant review, was a relatively simple system, the key points of which were the concepts of the 'drive', the 'trait' and the 'functional autonomy' of motives. A 'drive' was defined, (*Personality*, p. 113), as 'a vital impulse which leads to the reduction of some segmental organic tension. It has its origin in an internal organic stimulus of peculiar persistence growing characteristically stronger until the organism acts in such a way as to alleviate the accumulating tension'. The issues associated with evolutionary continuity and the nomothetic practice of referring human behaviour to a number of common instincts was thus avoided. He was impressed by the fact that many people live their entire life and fail to exhibit acquisitive, constructive, pugnacious or parental behaviour and in any case, as he later wrote (*Personality*, p. 320), 'If biological drive plays a part (thirst, hunger, sex) it does so not as *the* motive but merely as an irritable state of bodily tissues set within an intricate and personalized psychophysical system'—an integrated system moreover, 'in which the core of the impulse is not to be divorced from the images, idea of goal, past experience, capacities, and the style of conduct employed in obtaining the goal'.

Extension of the range of individual differences is made possible by the concept of *functional autonomy* of motives. A youth may enter upon the rigorous life of a seaman, which at first he finds irksome, merely to earn a livelihood; but as often happens, close acquaintance engenders a liking for the sea and he begins the serious study of oceanography, maritime law and navigation to satisfy his new sense of vocation. What began merely as a means to an end has now a motivation of its own. So, too, learning can not only extend all purposeful operations, but also give rise to new interests and new purposes and it is these contemporary motives which are the significant determinants of behaviour.

The most distinctive concept of Allport's system is, however, the *trait*, defined (*Personality*, p. 295) as 'a generalized and focalized neuropsychic system (peculiar to the individual), with the capacity to render many stimuli functionally equivalent, and to initiate and guide consistent (equivalent) forms of adaptive and expressive behaviour'. As distinguished from what he described as the biosocial view, which would regard the traits as mere names for categories of classification, Allport held (p. 289) that the trait has 'more than nominal existence, it is independent of the observer, it is *really there*', and further 'behind all the confusions of terms, behind the disagreement of judges and apart from errors and failures of empirical observation, there are none the less bona fide mental structures, in each personality that account for the consistency of its behaviour'. Consistently with his general position, traits were held to be unique to the pattern of the personality of each individual. Aggressiveness and asceticism, for example, while classifiable as such, would have some slight shades of difference in different persons. Attitudes, too, could be highly individualized and distinctive of the possessor and, like traits, are capable of determining behaviour; but attitudes have a definite point of reference. One has an attitude to or towards something. One could have a well-defined attitude towards capital punishment or legalized gambling, but a furtive, boisterous or considerate manner of behaviour would be a trait. As Allport emphasized, traits tend to be adverbial rather than

The most distinctive features of Allport's position are set out in his *Personality: A Psychological Interpretation* (1937) and in certain papers, most notably, 'The functional autonomy of motives', *Am. J. Psychol.* (1937), **50**, 141–156; 'Motivation and personality: a reply to Mr Bertocci', *Psychol. Rev.* (1940), **47**, 533–554; 'The ego in contemporary psychology', *Psychol. Rev.* (1943), **50**, 451–478; and 'Geneticism versus ego-structure in theories of personality', *Br. J. educ. Psychol.* (1946), **16**, 57–68. The application of the position was apparent in the works on prejudice and religion and was extended in *Becoming* (1955).

A principal difficulty with many of the explanatory systems current in the earlier years of this century was, for Allport, their method of referring present behaviour and attitudes to the individual's distant past. As he wrote (*Br. J. educ. Psychol.* (1946), **16**, 60): 'Psychoanalysis, behaviourism, hormic psychology, constitutional psychology and the preoccupations of the child psychologists, all favour a backward emphasis. They regard motives, say at the age of fifty, as elaborated, conditioned, sublimated, or otherwise modified, editions of instincts, drives or an Id whose structure "never changes". One might as well say that the pianistic dexterity of Myra Hess is an elaboration and extension of her original grasp reflex. Granted that there is a continuous evolution of her manual dexterity from the days when the grasp-reflex was her only digital stock in trade, is the energy that sustains her skill the aboriginal energy of the grasp? The grasp-reflex served one function in her life; musical dexterity a wholly different function. *Historical continuity does not mean functional continuity.*' Or again (*Am. J. Psychol.* (1937), **50**, 143): 'chronologically speaking, all adult purposes can be traced back to these seed-forms in infancy, but as the individual matures the tie is broken. Whatever bond remains, is historical, not functional.' The historical continuity between the life of the ancient city and modern Athens, in other words, can provide at best only part of the explanation of current stresses. It is the man who wants to support his commander now who is the reliable soldier. The relevant pattern of motivation is the contemporary one and between the seed forms of infancy and the present, several sentiments, attitudes and traits have been acquired. 'I think, therefore [he wrote in reply to Mr Bertocci; p. 535], that we may truly say that the infant is, *relative to the adult*, "pushed" by immediate, simple, vegetative, and proximate features of his hunger system. The adult is to a greater degree "pulled" by the delayed, ideational, non-proximate features in his system of hunger behaviour.'

It was at all times apparent that Allport favoured an *idiographic* approach to the explanation of human behaviour, one that emphasized the myriad possibilities of individual differences each personality acquires as it develops, in contrast to the nomothetic approach, which emphasizes what is common. He was sympathetic to the views of Burt (*Br. J. educ. Psychol.* (1943), **13**, 1–15) and of Bertocci who, influenced by McDougall, favoured the continuation of the search for the common elements in personality. 'Although I am aware of the argument made by Bertocci and others', he wrote (*Psychol. Rev.* (1940), 47, 540), 'that we "cannot give up the search for the common pattern underlying various purposes simply because our predecessors and contemporaries disagree about the number and kinds of irreducible unlearned motives", yet I am a bit pessimistic. Within 2000 years of self-conscious psychologizing no stencil to fit human desires has yet been found, because, I suspect,

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GORDON WILLARD ALLPORT (1897–1967)

Professor F. V. Smith writes: Professor G. W. Allport died of cancer of the lung in the Stillman Infirmary of Harvard University on 9 October 1967, at the age of sixty-nine. Apart from postgraduate work at the universities of Berlin, Hamburg and Cambridge, teaching appointments at Robert College in Istanbul and Dartmouth College, most of his professional life was associated with Harvard, where he graduated in 1919 with Phi Beta Kappa honours, took his Ph.D. in 1922, and later joined the teaching staff and was made a full professor in 1942. For several years he served as chairman of the Department of Psychology. In 1966 he was elected as Harvard's first Richard Clarke Cabot Professor of Social Ethics. He held honorary degrees from Colby College, Boston University, Ohio Wesleyan and Durham. He was an Honorary Fellow of the British Psychological Society and a past President of the American Psychological Association. In 1963 he received the Gold Medal Award of the American Psychological Foundation. He is survived by his wife Ada (née Gould), whom he met in graduate school, a son, Dr Robert Allport, a pediatrician working in Ross, California, and three grandchildren. Professor Floyd H. Allport is an elder brother.

The lasting impressions of Allport's contribution to the subject are the range of subject matter, the sustained, high scientific and literary quality, the high proportion of empirical studies and the distinctive independence of viewpoint. He wrote effectively on such diverse topics as eidetic imagery, factorial analysis applied to attitudes and personality, prejudice, liberalism, morale, rumour, personalistic psychology, the law of effect in learning and the psychology of religion. Apart from many journal articles and papers, his published works included: *A Study of Values* (with P. E. Vernon), 1931; a third edition with Gardner Lindzey, 1960; *Studies in Expressive Movement* (with P. E. Vernon), 1933; *The Psychology of Radio* (with Hadley Cantril), 1935; *Trait Names: A Psycho-Lexical Study* (with H. S. Odbert), 1936; *Personality: A Psychological Interpretation*, 1937; *The Use of Personal Documents in Psychological Science*, 1942; *The Psychology of Rumor* (with Leo Postman), 1947; *The Individual and His Religion*, 1950; *The Nature of Personality: Selected Papers*, 1950; *The Nature of Prejudice*, 1954; *Becoming*, 1955; *Pattern and Growth in Personality*, 1961; *Letters from Jenny*, 1965; and *The Person in Psychology*, 1968.

The reader of any of Allport's books or papers is always impressed by the breadth of reference, for he saw human behaviour within the whole context of tendencies that were innate and those acquired within a social context. He drew not only upon the literature of the many branches of psychology, of which in his own quiet and modest way he had an arresting command, but upon biography, history, ethics, educational philosophy, political theory, linguistics and sociology. His working life spanned active contributions by many of the well-known systematists including Dewey, McDougall, Watson, Stern, Angyal, Suttie, Tolman, Hull, Freud, Jung, Adler, Spranger, Köhler, Koffka, Lewin and Rogers; but while he wrote with understanding of a great variety of positions, his eclecticism was such that one was aware of someone patiently seeking to develop his own position, not to be different, but because for reasons always clearly and courteously stated, he found those of others less than completely satisfactory.

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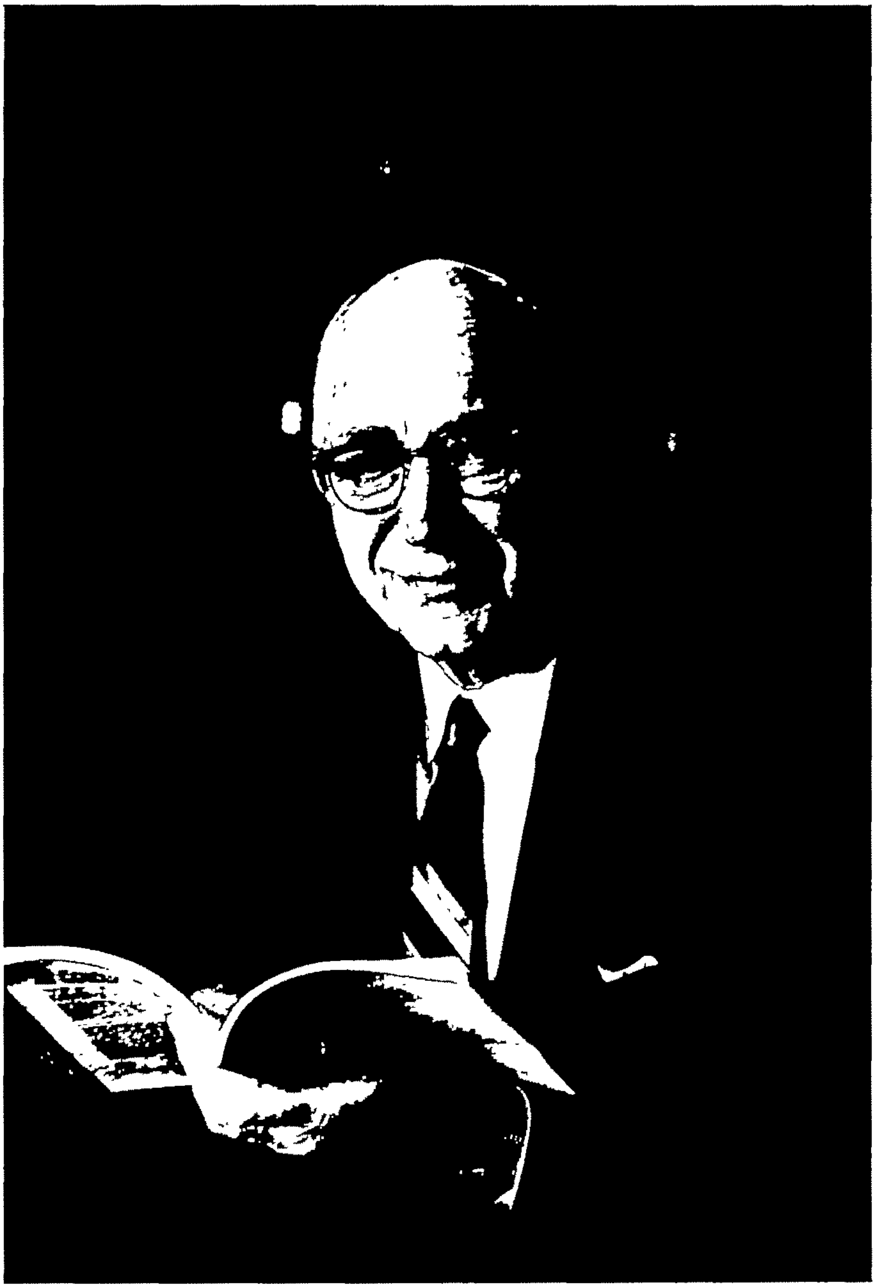

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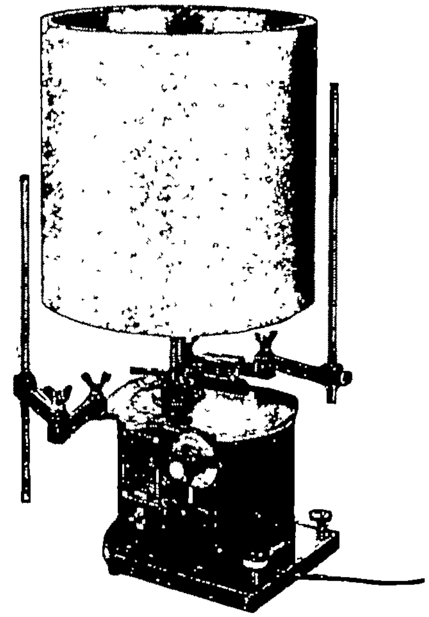

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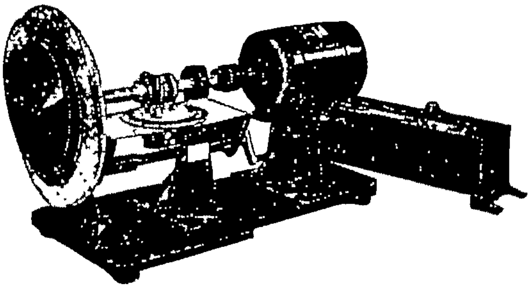

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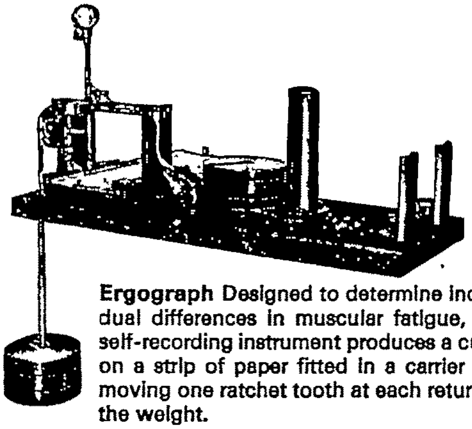

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application of the graph traverser to a variety of problem-solving tasks. It seems likely that in this field most of the advances must come in small steps by developing tricks to make machines solve first this problem and then that, though the different tricks often have a family resemblance.

One of the papers which does seem to contain a major new insight is that of Max Clowes on pattern recognition by machine. He believes that in order to build a machine to recognize wide varieties of patterns it is necessary to parse the input patterns in ways analogous to the syntactic grammars developed by Chomsky. This gives a rationale for the existence of feature detectors in the visual system of higher animals. It is of course true that the best way to perform a given piece of information-processing on present-day digital computers is not necessarily going to be the same as the method evolved in animals' brains, but there is a remarkable convergence between the ideas of Clowes and others working on the problem of pattern recognition by machine and the known functioning of the visual system in higher animals.

Another prediction made by Simon ten years ago was that by 1967 most theories in psychology would take the form of computer programmes, or of qualitative statements about the characteristics of computer programmes. Although Simon's time estimate was too optimistic, there can be little doubt that this must eventually happen. The only way to discover how a given information-processing system will perform in practice is to simulate it and find out by experiment—there are plenty of examples of this in the present volume. The existence of the digital computer not only allows the psychologists to discover how his theories really work; it also helps him to be rigorous in the way he formulates them since only a fully specified theory can be simulated.

Perusal of this volume may encourage psychologists to sharpen up their own theories and bring them to the point where their workings can be explored by simulation. Moreover, a knowledge of the tricks used in making machines perform intelligent tasks may suggest the sort of tricks evolved by the human brain to perform similar tasks. Even if the tricks are not the same, they are likely to be of the same general type.

This volume is the first of an annual series, and it is to be hoped that the series will help to bring home to psychologists what their hard-headed colleagues in artificial intelligence are up to.

N. S. SUTHERLAND

Interpersonal Accommodation. By MARK ABRAHAMSON. Princeton, New Jersey: Van Nostrand. 1966. Pp. 151. 14*s*.

Dissemination of interest in an area of growing importance in social psychology, namely patterns of human interaction, has been facilitated by the speedy availability of popular reviews such as Argyle's recent Pelican and the present paperback. They differ in their coverage, however. Abrahamson defines his concern as being with the mechanisms by which people adjust to each other, and draws frequently on material on other primates. First, he treats the unattached individual, and the treatment is a little curious in that he devotes little space to the description of overt social behaviour, reviewed fully by Argyle, but concentrates rather on part-processes such as response inhibition and vicarious learning, which on his analysis are contributory to accommodation mechanisms. Also included in this section is covert communication: here the treatment is somewhat uneven since again there is little coverage of ongoing experimental work but instead détours to bring in, for example, a reprinted article on awareness and a review of work on subliminal perception which gives undue space to the experimental preoccupations of ten years ago.

The second main section, on accommodation in groups, reviews more smoothly the relevant experimental work. While this work, on conformity for example, is more familiar, Abrahamson manages often to stimulate interest by new analyses and juxtapositions. He concludes with an all too abbreviated discussion of the motivational issues involved, which will, however, like much else in this book, provide useful discussion points for students. Since this, like others in the Insight series, is chiefly meritorious for its ability to lay foundations for more penetrating study, it is a pity that the student reader is not given a more extensive bibliography.

THELMA VENESS

Machine Intelligence. Edited by N. L. COLLINS and D. MICHIE. London: Oliver and Boyd. 1967. Pp. x+278. 63*s*.

As far as psychologists are concerned, most of the recent results on artificial intelligence are buried in arcane journals with titles like *Proc. 19th Nat. Conf. Ass. comput. Mach.* The present book makes readily accessible some of the results of recent work on artificial intelligence. The papers are based on the first Annual Machine Intelligence Workshop organized by the Experimental Programming Unit of the University of Edinburgh. As in most symposia, the contributors are a rather random selection, but most of the work reported is interesting and the volume gives a reasonable idea of the present state of the field.

The book inevitably invites comparison with Feigenbaum and Feldman's collection of articles 'Computers and Thought', published in 1961. The present papers are more technical, and few concessions are made to the general reader. This seems a pity since many of the detailed results have already been published elsewhere and there is a need for a volume such as this to brief non-computer scientists on recent advances in the field, stressing what is new about a particular approach. However, it is a little hard to rail at a book for not doing something it does not set out to do.

A second difference between this book and the Feigenbaum and Feldman collection is that, for better or worse, there is a less missionary spirit in the present papers. These are the writings of workaday scientists, not those of the apostles of a new creed. Progress in artificial intelligence over the last ten years has been slower than some of its exponents hoped. In 1957 Simon predicted that a digital computer would be the world's chess champion within ten years. The failure of such predictions may have made workers on artificial intelligence more careful about extrapolating, but it should not blind us to their solid achievements.

Much of the advance in recent years has been in the development of better programming languages in which to conduct work on artificial intelligence. As regards making machines carry out complex information-processing tasks there do not appear to have been many major new insights. The basic ideas in current use are not particularly novel—e.g. the use of evaluation functions to cut down the space which needs to be searched in order to determine the next move in a problem-solving situation. It is the detailed applications and the working out of ideas that are new, as in Burstall's work on the computer design of electricity networks, or Doran and Michie's a useful summary of Hull's intervening variable system, and a masterly introduction to the problem of demarcating between classical and instrumental conditioning. It would not be surprising to see some of these chapters make further appearances in other editors' books of readings. They amply deserve to.

W. P. BROWN

Principles of Psychological Measurement. By G. C. Helmstadter. London: Methuen. 1966. Pp. xx + 248. 36*s*.

For anyone concerned with the application, interpretation or development of tests of ability, attainment or personality, this book provides a lucid and economic introduction to the principles of psychological measurement. After general introduction, which includes a brief historical account of testing and a useful discussion of the nature of measurement and prediction, the main part of the book is a thorough and competent review of the essential requirements of any test, adequate standardization, reliability and validity. The rest of the book deals with the mechanics of test construction, with rating scales and inventories, and with the problems of multivariate prediction.

The author assumes a modest acquaintance with elementary descriptive statistics on the part of his readers, but even the unsophisticated in this field should not find the demands excessive. The book has much to commend it, not only to students or the test constructor, but also to teachers and educational administrators concerned with the application of tests of ability and achievement. If this book encourages them to take a long and critical look at some of the tests which come on the market it will have served a very useful purpose.

ELIZABETH D. FRASER

Group Conflict and Co-operation. By M. Sherif. London: Routledge and Kegan Paul. 1966. Pp. xv + 192. 21*s*.

The whole field of peace research and conflict resolution is now emerging as a new area of socio-scientific concern, to which psychologists such as Herbert Kelman, Charles Osgood, Otto Klineberg, Uri Bronfenbrenner, Daniel Katz, Ross Stagner, Tom Milburn and others are making substantial contributions. To these must now be added the name of Muzafer Sherif—already well known as a distinguished contributor to the field of small-group dynamics and reference group theory.

In this book Sherif has summarized the theories and results of his well-known boys'-club experiments in the fifties, and has applied these to the problems of international conflict. Though no new experiments are reported, social psychologists will welcome the clear restatement of the theory of superordinate goals which is the key to Sherif's argument. There are also some highly critical earlier chapters which deal with stereotypes, Freudian theories of aggression, and various other approaches to the origins of warfare, while smiting the strategists and deterrence advocates hip and thigh, and inevitably overstating the author's position.

The primary weakness of the book lies in Sherif's rash extension of evidence obtained from three (admittedly brilliant) experiments with boys of eleven or twelve to industrial and international conflict. His theory of superordinate goals must be given a place—alongside Osgood's graduated tension-reduction theory and several others—among the potentially important contributions to conflict resolution, but there is as yet no proof of its effectiveness in conflicts between states, and working out its possible application to the international scene suggests many difficulties and exceptions.

This is an angry, polemic, one-sided and sometimes repetitive book, even if one agrees with much that the author has to say. No doubt it is worth restating that modern war is not a catharsis of individual aggressive urges, that stereotypes and cognitions are greatly affected by values, attitudes and emotions, that cross-national contacts do not necessarily promote friendship, that deterrent measures are based on faulty psychology, that leaders are very restricted in what they can accomplish, and that even quite sane and sensible ones may lead their peoples to war. But readers from other disciplines might well have wished for a more balanced presentation.

Still, this is not so much a text-book as a challenge and a programme, and should be evaluated as such. Though we must not allow ourselves to be carried away, we can surely rejoice that another effort has been made from within our discipline to make a contribution to the most dangerous and compelling problem of our age.

A. N. OPPENHEIM

standable disregard for McLuhan, and without undue modesty, is the second revolutionary in the field. As one expects from the originator of the Q-sort technique, his latest book contains some brilliant insights, many controversial assertions, but certainly not a coherent theory of mass communication. The essence of what Stephenson has to say is contained in the following sentences: 'Social scientists have been busy, since the beginnings of mass communication research, trying to prove that the mass media have been sinful where they should have been good. It is my thesis that the daily withdrawal of people into the mass media in their after hours is a step in the *existential* direction, that is, a matter of subjectivity which invites freedom where there had been little or none before.'

This theme is discussed in a large variety of contexts, from advertising to national character, including a spirited defence of 'other direction' and supported by a large number of Q studies which are interpreted with enviable freedom; for example, when four factors emerge from a study of the function of newsreading—a ludenic factor, one of idealization, one of sensationalism and one of anxiety—Stephenson concludes: 'Only the ludenic factor, it seems to me, is being "truthful".' The book contains a recapitulation of Richardson's mathematical theory of politics which makes one weary of mathematical models, for after two pages of differential equations Stephenson says they indicate 'that the condition of least stability is one in which there are *n* equal states, all making war preparations against each other; the condition of greatest stability is when all but one nation has been reduced to zero size...' Who would ever have guessed?

And yet—perhaps Stephenson's theme that the mass media matter a lot in our lives as 'communication pleasure' and as an area of individual freedom is worth all of these 225 largely irritating pages. It will not do to say that it could have been put across better. Of course it could; but nobody did so.

MARIE JAHODA

Foundations of Conditioning and Learning. Edited by GREGORY A. KIMBLE. New York: Appleton-Century-Crofts. 1967. Pp. xii + 696. $8.75.

Those who know Dr Kimble's revision of 'Hilgard and Marquis' might make a pretty accurate guess at the contents of this related book of readings. Here the editor's own interests emerge even more strongly. The volume falls into two unequal parts. The first is devoted to historical, methodological and theoretical issues: the second to empirical papers on the expected themes—parameters of reinforcement and of generalization, motivational effects, inhibitory phenomena, the role of conditioning in complex processes and vice versa.

The emphasis is on conditioning rather than learning, and more on classical than instrumental conditioning. Thus the historical section deals with Sechenov (a most welcome chapter) and Pavlov, but neither Ebbinghaus nor Thorndike is mentioned. There is no name index, but if there were, Pavlov would obviously dominate it, with Hull a poor but clear second. Dr Kimble reprints two of Hull's papers (1929, 1938) and one by Tolman (1938), but Guthrie is represented only by Voeks' attempted formalization. There is nothing by, and very little about, Skinner. This is the major omission, although the (post-1939) contributions of Mowrer are also neglected, and we are assured that Spence is so influential that it would be superfluous to reprint him. On the experimental side, the selection of papers clearly implies that men condition classically (there are nine papers on the eyeblink) while rats condition instrumentally.

Books of readings are invariably disappointing. Dr Kimble agrees, but thinks he knows why. The trouble is that the papers which our students need to read are no longer being written, or at any rate published, since pressures of space force journal editors to insist on an excess of compression. So this book of readings is not a book of reprints. A quarter of it has been specially commissioned, and a further fifth is drawn from MSS. previously unpublished (at least *in extenso*); these are mainly Duke University theses. The policy seems to work, as the cut-down theses read well, and are quite as worthy of inclusion as the reprinted pieces.

The gems of the compilation, however, are Dr Kimble's own original contributions. They are characteristically clear, concise and important statements. Dr Kimble is perturbed by 'the growing willingness of theoretical psychologists to accept personal experience...as a basic datum—as if such experience were somehow not a concept but a *thing*'. To arrest this trend, Dr Kimble restates the 'basic tenet of behaviourism', that one must not confuse observation with interpretation. The implications of this are extended in a discussion of the operational definition of learning, and the importance of the learning–performance distinction. Dr Kimble also contributes

Developments in Psychoanalysis at Columbia University. Edited by G. S. GOLDMAN and D. SHAPIRO. New York: Hafner. 1966. Pp. xv + 357. $12.50.

This is an extremely difficult book to evaluate for two main reasons. First, it is not entirely obvious to anyone outside the psychoanalytic school what might constitute a 'development'; secondly, the content of the book is somewhat patchy, containing both good and bad contributions.

However, two features of the book seem to be of unusual interest where psychoanalytic writings are concerned. The earliest impression of 'developments' which occurred to this reviewer was the tendency to examine critically the usefulness—or even existence—of psychoanalytic concepts. The second was the disposition to turn toward experiment and quantification.

Neither of these features could be said to be especially well marked and, where the second is concerned, it is apparent that the mantle of 'science' is worn with some degree of discomfort. This latter point is exemplified in the chapter by Goldfarb, Levy and Myers, who feel that 'we ought not to limit ourselves to a few simple dimensions merely for the sake of methodological vigor' as such an approach can only be 'unrevealing of the true and essential significance [of what is being observed]'.

Similarly, in the chapter by Cooper and others, presumably it is unfamiliarity with statistical procedures which leads the investigators to quote statistical confidence at the 95 per cent instead of the more usual 5 per cent level. In this respect it is also pertinent to point out that, in a commentary on this paper, Knapp is obviously not entirely clear about the statistical procedures used by Cooper and his colleagues nor is he entirely sure what results were forthcoming from this experiment. My sympathies, in this case, are with Knapp.

Again, perhaps also reflecting the uneasy association with experiment and quantification, there is the apparently unorthodox meaning attached to the word 'science' in the chapter by Fleming. A scientific contribution presumably must represent something more than the statement of opinions or of speculative reasoning, but Fleming's definition would apparently not be in accord with this.

However, despite what has been said above, the book has a surprising degree of interest, probably because of its varied content. While some chapters are more 'typical' in the sense that they are concerned with theoretical issues and clinical case-studies, other contributions have a refreshing quality, especially, in the reviewer's opinion, those involving research topics and the anthropological study by Lubart.

No doubt this work will be of value to those working or training in the psychoanalytic approach; because of its varied content and broader scope it is likely to be of interest to a wider readership.

H. R. BEECH

The Play Theory of Mass Communication. By W. STEPHENSON. Chicago and London: University of Chicago Press. 1967. Pp. 225. 37*s*. 6*d*.

Research on mass communication reached a peak in the 'forties and 'fifties; since then it has been relatively stagnant. Workers in the field have for several years been talking about the need for a 'breakthrough', for there is a paradox in the situation: good, solid research has demonstrated that the newspapers do not 'win' elections, that personal influence is still a greater factor than mass communication in the formation and change of opinion, that television cannot be held responsible for increasing the crime rate—in short, that most of the common-sense views and prejudices about the impact of the mass media are without substance. But—and here is the paradox—the conclusion that apparently follows from much impeccable research that the mass media have not fundamentally influenced the quality of living in modern society is really unacceptable. Why has research not been able to demonstrate significant effects? Could it be because researchers have not asked significant questions?

It is on this front that signs of a breakthrough begin to appear. As always when traditions are rudely discarded, the new appears in unexpected quarters, confused and confusing—and yet, with all that, to be welcomed at worst as a challenge to defend traditional ways more adequately, at best as the beginning of a new approach.

In the field of mass communication the first revolutionary, McLuhan, has turned into a practical joker (it is debatable whether he ever was anything else). William Stephenson, with under-this experience. The central idea is fairly simple. The 'social innovative experimentalist', as he is somewhat irritatingly called, starts with naturalistic observations on a social problem, on the basis of which he defines the relevant parameters and generates hypotheses regarding more effective solutions. The next step is the crucial one: the creation of social 'sub-systems' embodying the new insights, which are then tested over a period of months or years to compare the outcomes of the new systems with existing ones. The whole enterprise is multidisciplinary, and Fairweather stresses the responsibility of the experimenter both towards the members of the sub-systems and for subsequently selling the new approach to policy-makers once it has been demonstrated to be effective. In practice, of course, the complexity of such a series of operations is enormous, and most of the book is devoted to spelling out the details.

As one goes through the book, one begins to wonder what kind of readership it is aimed at. On the one hand, there are highly technical descriptions of the subtleties of experimental design and analysis; on the other, one is told that 'the value of a correlation can vary from 1 to -1'! The job description of the chief investigator is described down to the way of avoiding conflict with his associates over questions of publication. There is also a somewhat disturbing *naiveté* about the manner of characterizing the role of the experimentalist *vis-à-vis* society, as though he were outside and (one is inclined to feel) above it. The work is full of such weaknesses, and yet one cannot help but admire the author for his dauntless grappling with innumerable difficulties. Not only is his heart in the right place, but he has done marvels in organizing multidisciplinary research. It is suggested that special centres ought to be set up where his methods could be applied to such problems as education, delinquency, drug addiction and race relations. Probably only a country as rich in human and material resources as the United States could attempt this. It is to be hoped that it will be attempted, and that further development will lead away from the hitherto somewhat narrow empiricism towards a more theoretical understanding of how such social sub-systems function and what they can achieve.

GUSTAV JAHODA

Interviewing: Its Forms and Functions. By STEPHEN A. RICHARDSON, BARBARA SNELL DOHRENWEND and DAVID KLEIN. New York and London: Basic Books. 1965. Pp. vii + 380. 40*s*.

One of the deadly sins of the reviewer is to criticize a book because its contents differ from what he anticipated from its title. This reviewer was tempted but resisted.

A large part of the book is concerned with advising and helping market-research and social-survey interviewers by passing on the winnowed wisdom of the authors. There are chapters chock-full of good sense based on plenty of experience of this kind of interviewing. Other sections carry first-rate discussions on the use of the leading question in interviewing, and some reported research work, carried out by the authors, suggests that the accepted warnings about its use should be re-examined. One of their investigations, carefully controlled, gave results which indicated that there was little or no distortion in the validity of the responses when leading questions were used in interview. (What bothered this reviewer a bit was the criteria used by the investigators to determine validity.) In any event the authors have pushed ajar a door that has, for many years, been considered closed. A thirty-page appendix by Dr Richardson on a study of selected personality characteristics of social-science field-workers is a model of clarity and sound methodology. The results of this study will be of interest to all social workers, particularly the finding that the most competent, experienced field-workers were found to have strong value judgements as reflected in the T.A.T. Another was that the skills of interviewing and observation were unrelated.

All concerned with interviewing will get some value from this book: the neophyte will get some good training, tips and know-how; the old hand will nod approval at the practical, helpful hints; sociologists, social anthropologists and psychologists who are planning or supervising surveys or designing researches will find parts of the book helpful.

DENIS MCMAHON

Manual of Psychophysiological Methods. Edited by P. H. VENABLES and I. MARTIN. Amsterdam: North-Holland. 1967. Pp. 557. 90*s*.

This book comprises a collection of authoritative statements on how to record almost every physiological measure currently of interest to the psychologist. Wisely the authors do not discuss the psychological significance of the various measures. Instead they concentrate upon telling the behavioural scientist what hardware he needs for the physiological recordings he is interested in, how to construct it or what to look for when buying, how to make the actual recording, and what artifacts to beware of.

Just as no one author could have written this book, so no single reviewer can be expected to criticize each chapter as an expert. What he can do, however, is to decide how successful the various specialists have been in conveying their knowledge to the layman, in this case the psychologist with a relatively limited physiological and technical background. Experts are not renowned for their ability in this respect and so it reflects much credit on the present contributors, and no doubt on the editors, that they do surprisingly well. What must have been two of the most difficult chapters to write, those on 'Electroencephalography' (Margerison, St John-Loe and Binnie) and 'Electronic Storage and Computer Analysis' (Ax), provide clear and fascinating reading. The former is the best short introduction to the EEG I have read. The latter, perhaps the most significant contribution in the whole book, is portentous for the future, promising or menacing according to how one views the possibilities of mass data reduction. 'Skin Resistance and Skin Potential' are covered clearly and competently by the editors, and at a length probably justified by the pitfalls attending this form of recording. Very lengthy also, but with perhaps less excuse, are the chapters on 'Electromyography' (Lippold) and 'Quantification of Biological Signals using Integration Techniques' (Shaw). Just as the latter title could have been simply 'Integration Techniques', so the whole chapter could have been shortened with little loss to the potential readership of this book. Brilliantly competent though is it, the same probably holds for the chapter on electromyography. Another contribution which makes rather heavy going is that on 'Pupillography' (Hakerem). This is due, not to its length or technical complexity, but simply to awkward exposition. For the rest of the book, to repeat, clarity and brevity are surprisingly good for a work of this nature. For those wishing to make their own equipment, many of the chapters include circuit diagrams with component values, nominated transistor types, etc., and others usually refer to sources of such circuits. Many of the contributors sound a proper note of caution concerning the dangers of artifacts, and give good advice on how to spot and prevent them. Most important of all, the techniques of electrode construction, choice and placement are exhaustively covered, even to the point of considerable repetition in different chapters.

Omissions? Everyone will have his own ideas, but it is difficult to understand why body temperature was ignored (except for a brief consideration of skin-temperature recording as part of the control for skin-resistance measurement). Guidance would have been welcome on methods of recording rectal and sublingual temperature and especially on the newer techniques involving tympanic and even nasopharyngeal sites. But this is a small criticism. I believe that most physiologically oriented psychologists will warmly welcome this book and find it a constant source of guidance and reference.

R. T. WILKINSON

Methods for Experimental Social Innovation. By GEORGE W. FAIRWEATHER. London: Wiley. 1967. Pp. x + 250. 60*s*.

The opening words of the book point an accusing finger at present-day social scientists (and this means me, the reviewer, as well as many of you, the readers). We are in 'a comfortable, middle-class position with role requirements stressing verbal behaviour', leaving the problems of social change to others. Calm reflexion, after the immediate defensive reaction, shows that there is much justification in the charge. Desire for social reform was at the origin of much of social science in Britain, but today, with the notable exception of some prominent sociologists, an excessive concern with real-life situations tends to be frowned upon. The sociologists dealing with poverty, moreover, remain close to the old tradition of straight fact-finding.

The author advocates a radically different approach, namely real-life experiments. He has himself conducted such research in the field of mental illness, and much of the book is based on

As well as this basic core of the model there is discussion of the tactile learning system in the octopus which has an organization much like the visual system. In both there are accessory structures in four lobes, concerned, it is said, with such things as holding the address of the analysing cells until the occurrence of reinforcement.

Professor Young is himself clearly anxious lest his model should seem 'absurdly simplified and "static" to those who are familiar with the subtle learning of higher animals' (p. 50), but if his picture really does represent the simple brain of the octopus it is a substantial contribution, and could, as he hopes, exhibit principles common to all nervous systems.

Not everybody, however, will agree with the conclusions about the brain of the octopus itself, derived as they are exclusively from lesions, behavioural experiments and microscopy. The argument is least convincing where interpretation of experiments in which lesions occur in the vertical and other accessory lobes is offered. Furthermore, Professor Young's diagrams of his model are unsatisfactory. They tend to consist of arrows coursing across representations of the lobe system of the octopus brain without any indication of what the signals which flow along the arrows do when they arrive at the arrow-points. This of course is the way the brain confronts the histologist and perhaps one should not therefore complain. These diagrams do not, however, constitute very satisfying explanation.

In spite of this the present book is, like the cephalopods themselves, not to be neglected. It summarizes an attempt which is rare, if not unique, in the field of neuroscience, to unravel the entire nervous system of a single species. In this work which has lasted now for more than 15 years, important pointers may have been discovered. Who is to say that the brain of the octopus will not do for our understanding of nerve networks what the giant fibre of the squid did for our understanding of the single nerve?

KEITH OATLEY

The Experimental Psychology of Original Thinking. By W. S. RAY. New York: Macmillan. 1967 Pp. xii + 208. 16*s.*

Readings in psychology (like elementary texts) are now all the rage and any new collection must justify its existence. The present one, published in Marx's *Critical Issues*, is aimed at the undergraduate. The book consists of seven chapters by the editor covering such topics as originality, set, and functional fixedness, followed by fifteen selected readings. The readings themselves are all independently interesting contributions. They include papers by Greenspoon, Maltzman, Mednick, Hull, Dewey, Poincaré and Luchins among others. (It is perhaps a pity that Frank Barron's original research on creativity is not represented.) But before getting to the readings the student meets the editor's introduction, which is diffuse, lacking in integration and far too detailed in exposition.

The diffuseness is caused by an indiscriminate borrowing of disparate theoretical concepts. 'Representational mediation processes', 'operant behaviour', 'pure-stimulus acts', 'habit family hierarchies', 'mediated generalization', 'cognitive operations' and 'response hierarchies' all do duty on a seemingly *ad hoc* basis to explain particular findings. The excess of detail is illustrated by a lengthy discussion of experimental minutiae concerned with the research which followed up Duncker's original experiments on functional fixedness. The climax to this section speaks for itself. 'Functional fixedness...is in the same position as other phenomena discussed in this book; the conditions that facilitate or inhibit it are not clear.' The specialist may conceivably find such an account illuminating; the undergraduate could hardly be blamed if he were to find it depressing. In fact the entire introduction presents what appears to be a conceptually confused area exploited by a rigorous methodology resulting in indeterminate conclusions. The psychological problems of thinking, and especially creativity, may be elusive, but at least they deserve a more sensitive and sophisticated consideration.

But perhaps we are expecting too much and should find consolation in the hints for setting up Maier's two-string problem. 'The strings must be hung from the ceiling (which is sometimes impossible with the high ceilings found in many old college buildings), several feet apart, and with enough space around them that they will not swing against the walls on any side. The distance between them has to be adjusted by trial and error, as does the length of the string, with due regard to the possible long legs of the S who is six feet, six inches tall.'

There is quite a good bibliography but no author index.

P. C. WASON

spatial framework. Again, the chapter on the effects of motivation on perception neglects the large body of experimental work on this subject, considering mainly Rorschach responses, hypnotic reactions and the Honi phenomenon, none of which are particularly relevant. A chapter on 'perception in modern technology' is principally on space flight. Finally, 'perception and the nervous system', though including Ratliffe and Hartline's work on *Limulus*, omits the important experiments of Hubel and Wiesel.

It is difficult, therefore, to discover whether the selection of topics and experimental data is based on any fundamental ideas as to the nature of perceptual problems. Moreover, issues are further confused by the introduction of extensive anecdotal data and hypothetical examples. Such material may be valuable in setting the scene for the beginner, but it can of course prove nothing. Furthermore, the author postulates that his readers should have some previous knowledge of psychology; thus they should hardly require such illustrative material. What they do need is some general arguments about the problems of perception, supported by relevant and appropriate experimental data. These would provide an introduction to the application of scientific method in psychological study. Although the book is clearly written and may stimulate the reader's interest, it will not encourage such an application. M. D. VERNON

The Memory System of the Brain. By J. Z. YOUNG. London: Oxford University Press. 1966. Pp. vi+128. 28*s*.

When worms learn (or not), when arthropods optokinetically rotate, when fishes have colour vision and amphibia regenerate their optic nerves, when birds peck keys and rats run mazes, when cats see a world composed of small straight lines, when monkeys grow up in difficult home circumstances and man creates yet newer intelligences, it is easy to overlook the contribution that molluscs make to our understanding of the workings of the nervous system. Yet the giant fibres of the squid have provided some of the most fundamental knowledge we have of nervous function. The discoverer of these giant axons was J. Z. Young, who in 1933 pointed out their possibilities.

In his latest book Professor Young proposes that the cephalopod mollusc can make a further contribution. The octopus, it is said, can help us to understand the function of complex nerve networks in perception and learning.

Those who are familiar with the author's *A Model of the Brain* will find little new in this book, which is a summary of the previous work. In some ways its brevity makes it less satisfactory than the earlier book, though it would be a good introduction to the work of Professor Young and his colleagues for those who did not already know it. One welcome innovation in the present book is the inclusion of photomicrographs to accompany the line-drawings and diagrams.

The central argument of the book starts with Craik's postulate that for an animal to be successful its brain must be a model of its environment, and represent neurally the events that occur outside. Professor Young then enquires what kind of model the brain is. The octopus is particularly suitable for this enquiry because it normally lurks in a cleft in the rocks and its motor behaviour largely consists either of emerging to attack some object, such as a crab, or staying where it is. A simple binary scheme also dominates the animal's visual abilities: it can easily discriminate between horizontal and vertical shapes. Finally, one of two things might occur as a result of its behaviour: the arrival of either food or pain. A typical experiment on this system is as follows. A shape, say a vertical rectangle, is displayed; then the animal may emerge to attack this object and be rewarded with a piece of fish or crab, or punished by an electric shock. The explanation is that the shape is categorized by cells in the optic lobe each of whose dendritic trees is arranged in a vertically or horizontally elongated oval. These cells contribute to analysing the vertical and horizontal extent of the stimulus shape. Axons from these classifying cells bifurcate, with one side going to a memory cell which orders the attack and the other to a similar cell which commands a retreat. Knowledge of results feeds-back to these cells in the form of signals representing either food (to the attack cell) or pain (to the retreat cell). Reception of one of these signals releases an inhibitory influence on the alternative pathway.

Professor Young proposes that this is the mechanism of the mnemon, the unit of memory. Learning is therefore a matter of elimination of available possibilities. In the octopus the situation is simple because there are only two alternatives, but the same principle of selection could apply to higher animals, and to man.

Bolles presents reinforcement theory, not as a novelty to confound or refute all previous theories of motivation but as a logical development and as a 'possibility'. Indeed his treatment of science is as a game—a game for an infinite number of players to play, where the prize is an increase in knowledge. With Alice he can say, 'It's a great huge game...that's being played—all over the world...Oh, what fun it is!' With this most important theoretical book, Bolles has shown his enjoyment of the game and has made a permissible move. Any player, however unconcerned for the prize, hopes to strengthen his position when he moves. Future players will no doubt show that this has been achieved, whether or not the prize is here.

ANNE BROADHURST

Assessment of Behavior. By J. E. HORROCKS. Columbus: Merrill. 1964. Pp. xv + 736. 96*s*.

Essentially, this book is a fairly comprehensive guide to a large number of the best known and most widely used psychological tests. These are grouped under the usual headings of intelligence, aptitude, achievement, maturation, personality, interests, attitudes and social behaviour, and the selection of tests for inclusion occasions little surprise.

A good deal of information is given about the selected tests: the history of the test, examples of items, details of administration, scoring, the age range for which it is appropriate and so on, and a great deal of attention is paid to the reliability and validity data which have been obtained for the test, or in some cases to the absence of such data.

The book, however, is much more than just a compendium of well-known tests. The first few chapters provide an essay on measurement and its place in science, a gentle introduction to simple descriptive statistics, and a discussion of the basic requirements of a good measuring instrument, while interspersed throughout the book are more specific discussions of topics associated with certain groups of tests. Thus the chapters dealing with tests of intelligence are preceded by a discussion of the nature and meaning of intelligence, and of the various approaches of those who have attempted to understand and measure it; the section on personality tests is prefaced by a review of the work of personality theorists from Freud to Eysenck; maturation and readiness are discussed, and 'culture-free' tests of intelligence, but, strangely enough, no mention is made of tests of creativity or of the controversy surrounding them.

This book is likely to be a useful general reference book for clinical and educational psychologists: it cannot, of course, in a mere 700 or so pages compare in comprehensiveness with the Mental Measurement Yearbook, but it does deal faithfully with a wide cross-section of currently used tests, and it does provide a useful background and generous bibliographies for users of these tests.

ELIZABETH D. FRASER

Visual Perception. By HERSCHEL W. LEIBOWITZ. New York and London: Macmillan. 1965. Pp. xii + 177. 16*s*.

This book is one of a series on 'Critical Issues in Psychology' which is designed to provide authoritative and provocative reviews of selected topics for the undergraduate reader. The author has selected eight aspects of perception for consideration, hoping to stimulate interest in these and create awareness of some fundamental problems and facts. His discussion forms the first part of the book; and the second part consists of 'readings', one to correspond to each aspect. These include useful papers by Boring, Zeigler and Leibowitz, Held and Schlank, Rock and Kaufmann, and Von Holtz.

Although the aspects selected are for the most part highly significant to our knowledge of visual perception, their discussion seems curiously disjointed and unsystematic. We begin with a chapter on 'adaptation to the perceptual world' which is confined almost entirely to the 'constancies'. 'Innate aspects of perception' are principally drawn from animal perception, though the experiments of Fantz and of Walk and Gibson are cited. But except in these there is little consideration of the origin and development of form and spatial perception, in this and in the following chapter on perceptual learning. Gestalt psychology is relegated to a chapter on illusions. 'Perceptual selectivity' and attention centre largely on the discussion of a fair-ground 'trick' perception of the horizontal and vertical which appears in fact to arise through adaptation to the from the University of Michigan, aim to present a clear picture of motivation as the experimental study of the determinants of man's actions for the enlightment of introductory students of psychology. And in this they succeed eminently. Their opening defines their terms and shows their very contemporary preference for a modified Hull/Lewin approach to action as reflecting the evocation of the strongest of competing tendencies, and using the concepts of availability, expectancy, incentive, motive, and consummatory value as determinants of the strength of an action tendency. Two further chapters deal with laboratory studies of these factors in motivation, and with day-to-day applications of the authors' seven incentive systems: sensory, curiosity, affiliative, aggressive, achievement, power and independence. Though never losing sight of the admirably selected experimental evidence, the emphasis at this point on human investigations and on real-life examples is particularly valuable for the beginning student who might otherwise be tempted to ask 'What does all this mean to an ordinary person like me?' The style is clear and this little book is certainly to be recommended for introductory courses.

But Birch and Veroff are not in the same league as Bolles. Bolles sets out to examine the concept of motivation, and the part it has played in past and current thinking. Motivation emerges strongly as a hypothetical cause of behaviour. In the first six chapters Bolles places this notion clearly in historical perspective, showing how it developed in early philosophical writings and fluctuated in strength or form in the writings of Freud, Lewin, Murray and the various instinct theorists before flowering in the drive concept as investigated by Cannon, Richter and Warden. This historical section culminates with an exposition of Hullian drive theory of motivation.

There follow four chapters reviewing the experimental evidence on the various hypothetical properties of Hull's drive construct D. Antecedent conditions, energizing effects, associative aspects of drive and its generality, as proposed by Hull, are all examined and found to be lacking experimental support, although the energizing effect fares best with much confirmatory evidence as well as much not in line with theoretical prediction. Nevertheless, Hull, as a fearless speculator and undoubtedly ingenious experimenter, receives due praise for his contribution to thought and his stimulus to research, to the extent of which these chapters, and yet another reviewing acquired drives, can testify.

Finally, Bolles discusses and reviews the evidence for alternatives to a drive theory of motivation. Various types of incentive theory (among which we must include the formulation of Birch and Veroff) are examined in detail, and a further study of the evidence from work on secondary reinforcement, avoidance and punishment leads us in the direction of reinforcement theory and away from motivation theory. In the final chapter the author modestly proposes 'to consider the possibility that the explanation of behaviour can proceed better without any motivational concepts, without involving needs or drives or wants or anything of the sort... that what we have called "motivated behavior" is neither more nor less than learned behaviour, and that all of the phenomena that have been called motivational can be translated... into phenomena of reinforcement'. Thus he revives and modernizes Thorndike's Law of Effect and brings evidence of the tenability of a reinforcement theory to explain behaviour. Bolles disposes of major objections to reinforcement theory by indicating that they have stemmed from a historically determined set to accept motivational theory and these two are incompatible. With the weaknesses of drive theory and incentive theory pointed out, the reader, skilfully led through the available evidence, must be convinced that drive theories require modification and that reinforcement theory offers a parsimonious alternative with few major difficulties.

So bare an outline of this important book can hardly convey the rare pleasure that it is to read. There is enjoyment of a style which combines scholarship with the casual aside, encouraging the impression that the author is speaking, or rather conducting a seminar, for the reader alone. There is pleasure also in having an opportunity to explore with the author, and always with sympathetic understanding, the logical problems of the theorizing of others. For the teacher of motivation there are admirable summaries of the positions of previous workers in the field—and they are all here, great and small, too numerous to mention in a review. Each is discussed, genuine contributions with retrograde steps, strengths and weaknesses, and all in relation to others. The tautologies, circularities and paradoxes must be tenderly displayed because it is from previous weaknesses that present strengths will grow. For the research worker there is the formulation of a rich store of unanswered questions not only in the theory here put forward but in the careful analyses of other theories.

to Music' covers physiological responses, reports of mood change, the effect of repetition of music on reports of enjoyment and various measures of change in musical taste. The chapter on currently available tests of musical behaviour is a useful survey. Surprisingly, there is no mention of musical creativity. The section on the therapeutic use of music in psychiatry is of interest, but if Dr Lundin's accounts are complete there is too little data as yet to allow adequate evaluation.

There are a few criticisms of a minor nature. The terminology is not always clear. Music students may have difficulty with the term 'absolute DL'. A graph is labelled with a decibel scale before any mention of the term is made in the text. Finally, the Elizabethan composer's name was not Charles but William Byrd.

ANN D. M. DAVIES

Human Learning and Memory: Selected Readings. Edited by NORMAN J. SLAMECKA. New York: Wiley. 1967. Pp. x + 543. $5.75.

'This book makes available a substantial collection of some of the significant research articles in the area of human learning and memory.' Thus Slamecka introduces this book of fifty readings: ten sections of five articles, each section dealing with a specific research topic and including a brief introduction and five additional references. These latter tend to be to review articles, while the reprinted papers comprise original research reports or theoretical contributions. All are relatively recent in date, most of them having appeared within the last ten years and a good number within the last five. They are concerned virtually exclusively with verbal learning and with laboratory research; the book has in fact been designed principally for use in advanced undergraduate and graduate courses in verbal learning.

The selection of topics, as well as of articles, must in the last analysis be arbitrary and cannot suit everyone. It is curious, for example, to find an entire section devoted to the serial position effect as well as another on serial learning processes, while psycholinguistics is represented by a single section in which there are no articles dealing with language behaviour in children. Again, a single section on concept formation gives cavalier treatment to that topic as well as providing the only mention of problem-solving. Such omissions are surely not dictated by a dearth of recent work employing experimental techniques. These reservations aside, the book furnishes an extremely useful orientation to recent work in the areas chosen for consideration; the introductions to sections are admirably brief and to the point, and the articles reprinted are such as appear likely to become classics.

One is hampered in the final appraisal of this book by a certain doubt as to the purpose of books of readings in general. Whether or not a selection is used as a 'set book' in teaching, its function is presumably to simplify, and indeed largely to replace, recourse to the journals (perhaps undesirable in principle but necessary in practice). Apart from highly specific purposes of analysis and replication, I can think of two reasons for consulting journals: to acquire up-to-date knowledge in a particular area, and to gain a second-hand 'knowledge of acquaintance' of that area—to get the feel of the topic by reading original research reports. If the former were the chief purpose of this book, it would be better served by printing just those review articles which were on principle excluded or quoted only as additional references; they provide more information and are rather less likely to date. If the purpose were to give the 'flavour' of research, why not include earlier classics and (by contemporary lights) 'wrong answers'? Nevertheless, this book goes a long way towards meeting both needs, and justifies the claim made for it by its editor.

ANN TAYLOR

Motivation: a Study of Action. By D. BIRCH and J. VEROFF. Belmont, California: Brooks/Cole. 1967. Pp. ix + 98. 12*s*.

Theory of Motivation. By R. C. BOLLES. New York: Harper and Row. 1967. Pp. 546. 45*s*.

Here are two books to remind us that we should always look beyond book titles and go to the authors and publishers, even when in search of first impressions. *Motivation: a Study of Action* by Birch and Veroff must surely contain a proportion of theory and so partly encompass Bolles' *Theory of Motivation.* It does, but we have here two treatises so disparate in aim and treatment that to consider them together risks doing justice to neither.

Birch and Veroff, writing for the 'Basic Concepts in Psychology' series which emanates largely the transmission of cognitive skills through language must remain obscure. Meanwhile it is worth remarking that 'transmission is a dangerous term in this context because it tends to be linked with the highly misleading notion that 'learning with the help of language' is synonymous with 'being told'.

A short comment on the translation is now perhaps called for. On the whole it is very clear and fluent but there are one or two places where the English rendering is sufficiently curious to make recourse to the French seem necessary. For instance, on page 289 the English version reads: 'the actual development of language, about which we unfortunately have no knowledge...' As it stands, this is an extreme and unwarranted statement; but it turns out that the French phrase in this context might be more appropriately translated as '...about which we are unfortunately not well informed'. An even odder example is to be found on page 158, where 'C'est forcé' is translated by 'It is forced'. The fitting translation would be something like 'it must be'—the context being a child's comment on the necessity of transitivity in certain relationships. However, these are minor points, and the translation as a whole seems excellent.

In summary, it is probably true to say that Piaget's general position has never been stated with more force and clarity than in this work, and that the relation of his arguments to those of Beth makes the book one of quite exceptional interest and importance.

MARGARET DONALDSON

An Objective Psychology of Music, 2nd edition. By ROBERT W. LUNDIN. New York: Ronald Press. 1967. Pp. vii+345. $6.50.

This book is a revision of a text that first appeared in 1953. It is offered to two groups of students, 'to students of psychology who want to find out more about musical behaviour and to students of music whose knowledge of human responses to musical stimuli needs a scientific basis'. There are three chapters on the dimensions of tone, three on melody, harmony, consonance and rhythm, and further chapters entitled 'Learning and Remembering Music', 'The Affective Response to Music', 'The Aesthetic Response to Music', 'The Nature of Musical Ability' and 'Musical Performance'. There are two chapters on the measurement of musical behaviour and the book closes with a survey of the uses of music in industry and therapy. The book contains no musical examples.

The author wants to give students 'an approach toward musical behaviour, stripped of its mentalistic concepts, that will be in keeping with contemporary psychology'. He deplores the mentalistic approach exemplified by the texts of Seashore, and points out that 'the objective approach, shown in the reporting of only the most empirical studies, results in a clear understanding of the factors operating in musical responses'. But Dr Lundin's book is no *Plaine and Easie Introduction*. Much of his discussion is neither objective nor empirical. In talking about the role of rhythm, for instance, he says: 'Compare a Gregorian chant, which is monorhythmic (one rhythmic line), and almost any orchestral composition, where several rhythmic lines occur simultaneously. The latter is far more musical.' Again, on page 188 we are told that popular songs have less intrinsic worth than much serious music. Many empirical studies are omitted too; in particular, all those published in the musical sections of the acoustical journals since the 1940s, so that there are gaps in his treatment of consonance and dissonance, timbre and absolute pitch.

The author's over-concern with fighting dualism in psychology interferes with clear exposition. He objects to our characterizing sound as having physical and psychological attributes 'because we may unwittingly fall into a psychophysical parallelism, a dualistic dichotomy so typical of mentalistic thinking'. The terms 'vibrational aspect' and 'tonal aspect' are to be used instead. The tonal attribute, pitch, is 'a property of the stimulus and not a product of the individual's reaction, as is so often implied by our mentalistic thinkers. True, one may *report* a pitch as being high or low. That is the individual's response to a quality of the sound which exists. But the pitch quality of highness or lowness exists as an attribute of the stimulus regardless of whether or not there is someone around to tell us that. Dr Lundin gives little account of scaling procedures and nothing at all on context effects. Later on he poses the query 'How many different pitches can the average individual discriminate?' but to give as an answer 1800 at 60 db whilst neglecting to say that only 5–6 pitches can be reliably identified by the method of absolute judgement is misleading to musician and psychologist alike.

The later chapters in the book are more satisfactory. The chapter on 'The Affective Response important qualification, however, that we do not have to regard intelligence as a 'more or less mysterious divinatory power' and points out that machines which can simulate intelligent behaviour not fully governed by deductive rules are already a reality.

Piaget's part of the book begins by emphasizing—and in this he agrees fully with Beth—that logico-mathematical studies and psychology are entirely independent, the former being concerned with questions of validity, the latter with questions of fact and of explanation. However, the need for co-ordination of these studies becomes evident as soon as epistemological questions are raised, since epistemology is concerned with the relations between the subject as knower and the object known. Further, in considering the object, it is necessary to distinguish questions having to do with the properties or 'forms' of the object and questions concerning the type of existence or reality to be attributed to the object. On the basis of these distinctions Piaget offers a very lucid analysis of the methodological principles which he suggests should govern epistemological inquiry.

He then proceeds to an examination of the contribution of psychology and makes immediately the point that only a genetic or developmental psychology can hope to provide relevant facts. For example, one may consider the crucial epistemological question of the status of self-evidence. The history of mathematics shows clearly that self-evidence does not have the absolute and unchanging character once attributed to it. It has become necessary to think rather, as Beth points out (following Bernays), of changing or 'acquired' self-evidence. But in this case the question necessarily arises of how such 'acquisition' comes about and here only genetic psychology can possibly have any solution to offer.

Piaget's answer to this question and to other related questions of acquisitionis given in terms already quite familiar to those who have studied his more specifically psychological writings. He argues, in brief, that all logico-mathematical experience derives not from *a priori* forms, nor yet from experience of physical objects, but from the co-ordination of the subject's own actions; that the universal nature of this experience (its invariability from subject to subject) arises from the fact that it is the most general co-ordinations of actions (of ordering, combining, etc.) that are involved; and that the roots of these must evidently be sought in general principles of biological organization. We come close to the notion that is at the core of all Piaget's work in the following quotation: 'Our hypothesis then is that there are elementary structures common to all living subjects and that the creation of forms by intelligence prolongs organic morphogenesis.' It is in this way that he hopes to show that these forms are both genuinely constructed and at the same time non-arbitrary—hence neither strictly discovered nor yet freely invented. So the course of development is seen as a succession of structurings, and internalized representations of structurings, each one of which opens up whole systems of possibilities, some of which are then actualized by later structures. We can therefore speak of a 'framework of predetermined possibilities' and it is clear that the development can never in principle reach a final end. Piaget very interestingly relates his claims about the progressive development of cognitive structures, each one coming to be integrated into a larger and more powerful system, to the Gödelian argument that the non-contradiction of a system can only be demonstrated by having recourse to a stronger system; and he suggests 'convergence' in this respect between the formal and the genetic. In particular, he takes Gödel's results to suggest that ontogenetic construction is (*a*) never-ending and (*b*) constrained to proceed along certain paths because of the power of the effort to eliminate contradiction (which Piaget identifies with the tendency towards increasing equilibrium)—an endeavour that can never terminate in total success.

So far, in this necessarily very brief account of how Piaget conceives of the relations between genetic psychology and epistemology, no mention has been made of the connexion between thought and language. Yet this is obviously a crucial question. In the context of this book, Piaget considers the topic mainly with reference to attempts to found logic and mathematics on socially agreed conventions which would then have to be thought of as 'transmitted' through language. Piaget is prepared to allow—now as previously—that language is a necessary condition for the achievement of advanced levels of formal thinking, but he denies that is is a sufficient condition at any level. The claim that it is sufficient would certainly be an extreme one to make and very hard to justify by any available psychological data. What seems evident is that from early stages in ontogenesis there are interactions between the acquisition of language and the development of thinking. The precise nature of these interactions is still quite unclear but it is probable that they are of very considerable complexity. Until we know much more about them, the notion of

Weiskrantz, writing chiefly about the effects of electro-convulsive shock in animals, brings out very clearly the importance of assessing the exact implications of experiments before deciding which memory process has in fact been impaired. Whitty & Lishman bring out the complexity of the disease processes rather at the expense of the complexity of the psychological disorders. Zangwill distinguishes the Korsakoff syndrome from the amnesic syndrome. He conveys some of the flavour of the raw material of the book. The effects of head injury are set out neatly by Whitty & Zangwill but they might make more of the phenomenon of loss of temporal sign in mild disorders. Milner gives good expositions of some of the famous cases where surgery produced disastrous memory defects and also of the technique of anaesthetizing one hemisphere at a time to prevent it happening again. She suggests that the memory defect is one of consolidation and thus seems to implicate storage rather than retrieval, an assumption which seems a shade uncritical in the light of Weiskrantz's first chapter. Williams discusses the confusing evidence of the effects of E.C.T. on memory: she could put more emphasis on the evidence that material may be retained even when retrieval appears impossible. Brierley is tough-minded about the neuropathology of these disorders: unfortunately, he makes tough demands on the reader's knowledge of neuroanatomy. Stengel uses his few pages to remind us that neither organic nor hysterical disorder immunizes against the other. Unfortunately he has space for only a very brief introduction to psychogenic memory disorders themselves. In particular it would have been useful if he had given a fuller discussion of the point that hysteria is not equivalent to malingering, since the final chapter by Gibbens & Williams is much concerned with court decisions on 'automatic' behaviour.

At the end one is left with two wishes: first, that this book were better organized; second, that before much longer the experimental psychologist would succeed in penetrating the clinic and consummating what promises to be so fertile a union. Or perhaps we could hope for a *ménage à trois* with the psychometrician.

P. R. F. CLARKE

Mathematical Epistemology and Psychology. By EVERT W. BETH and JEAN PIAGET. Translated from the French by W. MAYS. Dordrecht, Holland: Reidel. 1966. Pp. xxi+326. 105*s*.

This book is divided into two major and quite distinct parts. The first, by Beth, is largely a survey of the history of philosophical arguments concerning the foundations of logic and mathematics, a survey in which Beth deliberately chooses not to make a strong defence of any particular viewpoint, since he considers that all traditional notions on the subject are at presnt inadequate and that some sort of 'doctrinal synthesis' must be looked for. The second part of the book is a discussion by Piaget of the relations between logic, psychology and epistemology, with special reference to the problems involved in a consideration of logico-mathematical thinking. Finally there is a brief concluding section to which both authors subscribe and in which their epistemological position is summarized.

The historical perspective which Beth provides is necessary if the issues later taken up by Piaget are to be understood. In particular it is essential to appreciate the significance of (*a*) traditional arguments concerning the status of self-evidence or intuition as a foundation for the development of pure mathematics; (*b*) related arguments about the status of mathematical entities—that is, whether they must in some sense 'really exist' and consequently whether the result of original work in mathematics is to be regarded as invention or as discovery; and (*c*) the search for a means of guaranteeing the non-contradictoriness of deductive theories, which becomes essential if the appeal to self-evidence is disallowed. This last issue reached the proportions of crisis when Gödel demonstrated the impossibility of proving the consistency of certain formal systems from within the systems themselves.

It is also important to note Beth's emphasis on the traditional distinction between heuristic and demonstrative procedures. Beth argues that only a general solution of the decision problem—which can be shown to be impossible on the basis of Gödel's results—would allow this division to be fully overcome. He then goes on in the final chapter of his section of the book to consider the construction of thinking machines, and he reaches the conclusion that 'it will always be necessary to have recourse to intelligence' and hence never possible to construct a machine capable of solving all problems. A brief inserted comment by Jean-Blaise Grize makes the by tending to buy less of it, and still less when given a greater amount of money to do so; with men shoppers, however, the results were essentially in the opposite direction and, though not predicted, Brehn explains this disparity by the contention that the choice of items in a food store is a more important freedom for women than for men, hence there is greater reactance from the women.

Granting its plausibility in explaining some phenomena, and the admirable way in which Brehm presents its features and ramifications, the theory of reactance as it now stands is only a partial theory. For one thing it is essentially an explanatory construct and, like cognitive dissonance with which it is linked in both a general and a particular way, it is not fully adapted to handling findings which are contrary to its guiding postulate. Accordingly, it must explain these in a *post hoc* way consistent with that postulate, as in the instance just noted of the male shoppers. In at least one of his experiments, Brehm finds a 'sour grapes' effect—a decrease in attractiveness for an eliminated alternative—and to handle this he observes that it '...will tend to subtract from reactance effects'. But he offers little further to suggest what predictions can be made from the theory about when and how much this will occur. Surely this is a crucial point since many conceptions about frustration and choice involving conflict, harking back to Freud and to Lewin, imply that an eliminated alternative is reduced in valence, through rationalization or whatever mechanism. There are also methodological problems here, associated, for instance, with the assumption that low compliance to a 'high pressure' threat demonstrates reactance when it may be merely annoyance with an unnecessarily clumsy instruction, e.g. 'We are sure that you will be greatly influenced by the opinions stated, and that your answers this time will tend toward those of this student.' The triviality of many of the choices afforded in these experiments also raises questions about whether comparable findings would emerge from a study of eliminated alternatives that were more crucial to the person's self-concept and that implicated pride, shame, or embarrassment in ongoing relationships.

Nevertheless, much the most important contribution of this book is to present a set of interrelated concepts and research reports in a highly systematic fashion. Brehm's detailing of what he is up to, within his terms of reference, is exceedingly clear throughout, and he shows a commendable willingness to point out and consider some of the loci of difficulty along the way. The book therefore stands as a worthwhile and instructive one for advanced students in the field.

EDWIN P. HOLLANDER

Amnesia. C. W. M. WHITTY and O. L. ZANGWILL. London: Butterworths. 1966. Pp. x+217. 64*s*.

This book should be studied carefully by anyone who feels tempted to outline a theory of memory and, above all, by anyone who has ever felt that the label 'loss of recent memory' is a satisfactory description for a patient who has difficulty in remembering. Unfortunately, to get the best out of it the reader would need to be something of a polymath: one of the virtues or vices of the book is that the ten experts do not make concessions to those illiterate in their separate fields. One defect of multiple authorship is the amount of independent replication from chapter to chapter: this sometimes serves the useful purpose of highlighting different aspects of the same evidence, including different conclusions drawn from it; at times it reveals how little interdisciplinary communication has occurred hitherto, for most of the authors feel it necessary to give their own definitions of memory processes. One wishes that all the authors had had the opportunity of reading all the other contributions before writing their own! Of course, without multiple authorship it would be difficult to give such coverage and variety in style, from very detailed analysis of the implications of experimental findings to clinical descriptions of cases and simple exposition of well-established clinical phenomena.

However, there is further cause for complaint in the disproportionate space devoted to some aspects of memory disorder. While the organic memory disorders deserve their six chapters (and more), psychogenic disorders should surely have more than one chapter in a book with this title. There is perhaps also an over-emphasis on the disease processes involved at the expense of the phenomenology of the memory disorders themselves. In particular the gross inadequacy of psychometric techniques for assessing any sort of memory disorder almost escapes comment. However, let us be thankful for what we get.

and the same determination, which recurs throughout the volume, to anchor the private in the public, if not to reduce it to it—runs on into Williams's masterly discussion of the difficulties of trying to identify persons by non-bodily criteria.

Austin's manifesto of 'ordinary language philosophy' opens the volume; Strawson's example of 'descriptive metaphysics' concludes it. These, and what falls between them, provide a balanced picture of the diversity of contemporary Philosophy of Mind.

J. P. WHITE

The Psychology of Human Ageing. By D. B. BROMLEY. Penguin. 1966. Pp. 366. 6*s*.

Here is excellent value for six shillings: an original text spanning nearly every aspect of human ageing. The fourteen chapters cover the biological, pathological and psychological aspects of ageing. Ten chapters deal directly with the psychology of ageing and include separate consideration of the social, occupational and methodological aspects. The bibliography is comprehensive; a worthwhile acquisition in its own right.

A major problem with a book like this must be to decide the breadth and level of the readership for which the book is intended. Bromley writes: '...the book should meet the needs of undergraduates and certain post-graduate students, especially in psychology', and it '...will interest the "intelligent layman"'. The book falls short as a text-book of the psychology of ageing, however, in its presentation and style. Paragraphs a page long and a dearth of visual complement to the text reduce its value for easy study or reference.

Equally it will try the patience of the 'layman', who will be confused by passages of technical language and irritated by the presumption of a basic knowledge of psychology. In between patches of readability are presented simplified concepts veiled in technicalities.

Bromley succeeds in steering a middle course between the technical and the popular for much of the way. He gets off course from time to time, particularly in the sections on cognitive and occupational psychology. It is interesting to compare passages with the 'classic' *Ageing and Human Skill* (Welford, 1958). Welford's book is unrelentingly hard going; the line of thought has to be traced through a mass of experimental results; but Bromley's abstraction of the thought is not always clearer or more edifying. For the informed reader, however, here is an overall view of the field which will direct and stimulate further reading, and facilitate the acquisition of a balanced perspective in approaching the subject of ageing. For those who are already well read in the subject, Bromley's book will stimulate critical appraisal and a comparison of references. The student must be warned that reading Bromley does not absolve him from reading the hard books, and the bibliography gives him no excuse for evading them.

ROGER MOTTRAM

A Theory of Psychological Reactance. By JACK W. BREHM. New York: Academic Press. 1966. Pp. x + 135. 48*s*.

In this monograph, Professor Brehm presents a set of concepts and related research studies surrounding the effects of restrictions on an individual's freedom. His particular focus is directed to those 'free behaviours' over which persons expect to have control and in which the choice alternatives are qualitatively different. His guiding postulate is that when such a freedom is threatened or eliminated, a motive is aroused to restore it called 'psychological reactance'. Other features of the theory concern the effects of impersonal versus personal sources of threat, the importance of the freedom to the person, how much of it is threatened, and whether or not the circumstance of threat is revokable. Some of the predictions he treats are: that a choice alternative which is threatened becomes more attractive, and that this 'magnitude of reactance' is increased by providing only a small number of other alternatives or by having some personal source select an alternative for the individual.

For the most part, the considerable number of experiments reported here involve children or adolescents in situations that are aimed at arousing an expectation of freedom of choice which is then thwarted. The matters of choice concern preferred gramophone records, toys, and candy bars, and in these studies the findings rather consistently show that *making* the subject choose a preferred alternative *decreases* its attractiveness on subsequent ratings while the attractiveness of other alternatives increases. This negative influence is also revealed in a field experiment with women shoppers in a supermarket who reacted to a 'high pressure' appeal to buy a certain bread ality—those of Adorno *et al.* in *The Authoritarian Personality*, and those of D. Katz in a 1960 article. The chapter on attitude change discusses various features of a communication that may make it more or less persuasive, and various features of viewers that may make them more or less susceptible to persuasion, and ends with a discussion of some models of attitude change based on ideas of congruity, balance and cognitive dissonance.

All this amounts to a series of reminders about what has recently been claimed by theorists, together with occasional comments about gaps in the evidence, and one can easily imagine the value of this to the Committee. One can also see that the members of the Committee have no need for explanations of words like 'personality', 'belief system', 'dimension', and so on, or for reminders of the foundations laid before 1950. But I think that in spite of the appendix on research methods put in for their benefit, the interested laymen, students, and television professionals may find the book rather baffling.

D. M. BARKLA

Essays in Philosophical Psychology. Edited by D. A. GUSTAFSON. London: Macmillan. 1967. Pp. x+413. Cloth 30*s*. Paper 15*s*.

This is a most useful collection of twenty celebrated essays on Philosophy of Mind, published between 1949 and 1962. Only one of them, by Farrell, is specifically concerned with a problem in the psychological sphere; the logical difficulties in establishing criteria for a psychoanalytic interpretation. But the other papers should also be of great interest to psychologists.

Most of the articles are devoted to the clarification of particular mental concepts, especially those to do with action, emotion, imagination, memory, pleasure, attention, dreaming. The editor has wisely not aimed at comprehensiveness, but at revealing the many-sidedness of contemporary analyses of a limited number of problems.

The analyses are interrelated by a common interest in the thesis that mental terms do not pick out private processes but have a more complicated logic. Bedford, for instance, argues that emotion words are not the names of feelings, but words used to interpret and assess behaviour: their primary function is judicial, not informative. Benjamin denies the old view that 'remembering' labels a mental state, introspectively different from imagining: it is not—or is not primarily—a 'process' word, but an 'achievement' word, of the same family as 'knowing'.

The same theme runs through four other topics—action, imagination, pleasure and dreaming—which each cover several essays. On 'action', Vesey argues that one cannot analyse actions in terms of volitions which cause bodily movements. Melden stresses the connexion between actions and social rules, Anscombe the difference between causal explanations of actions and explanations in terms of intentions and motives. Austin explores the subtle variations in meaning between actions done 'unintentionally', 'involuntarily', 'accidentally', etc.

Among all these four writers there is no radical disagreement: each charts a somewhat different area. Over the other three topics there is rather more controversy.

On 'imagining', Shorter agrees with Ryle that it is not like seeing mental pictures, but disagrees that it is like pretending (and therefore not different in kind from overt activity). Austin's careful unpicking of 'pretending' is also at odds with Ryle (while explicitly directed against Bedford).

On 'pleasure', Ryle again. Pleasures are not episodes, but forms of attending or heeding—concepts which are to be analysed largely dispositionally. His two opponents are Penelhum and Place, the first defending pleasure episodes, the second, with a broader sweep, attacking the dispositional analysis of heed concepts in general.

Malcolm, on 'dreaming', makes the boldest onslaught on privacy: dreams are not experiences during sleep: we can never get further, in our understanding of dreaming, than the dream report. Pears makes a detailed counter-attack—but, unfortunately (and this is one of the few weaknesses of the collection), much of his argument will be lost on those who do not read the whole of Malcolm's book *Dreaming*. Macdonald argues that Descartes was wrong in classifying dreams as illusions, indistinguishable from waking experience.

The last four essays concern more general problems. Strawson, Ayer and Malcolm provide three highly original and very different answers to the 'other minds' problem. Strawson's solution comes in the middle of his argument for a 'logically primitive' concept of a person, collecting attributes of both mind and body, but irreducible to either. This metaphysical strain—

They base their claims on a study of sixty-seven subjects, 17–62 years of age. Besides twenty healthy subjects the group included hospital patients who were undergoing various forms of drug treatment. What may have influenced the selection of this heterogeneous group is not disclosed, nor do the authors explain why some of the healthy subjects were occasionally given drugs during the test.

Their light source was an electronically driven 'visual indicator tube'. The authors regret that its intensity and spectral composition could not be specified. They add that 'as the light was green, rays of 500–520 mμ wave-length had to be prevalent, the intensity of which, obviously, exceeded the lower threshold value of photopic vision'(!). The flicker properties of the light source were evidently not calibrated, so that the stimulus frequency, as distinct from the frequency read from the dial of the 'impulse generator', cannot be determined. No attempt was made to control the adaptation of the eye or other extraneous sources of 'serial' variation in the CFF.

Although the authors promise CFFSE data for all 67 of their subjects, data are in fact given for only the 42 subjects included in the 'statistical survey'. The remaining 25 were left out because their data were 'extreme' or considered inappropriate in other, unspecified, ways. The statistical analysis is of the most cursory kind, and to test the authors' claims the reader must plough through 150 pages of individual CFFSE curves in the appendix.

Visual inspection of this vast mass of data lends some support to the authors' claim concerning the existence of non-random variation in CFFS—but only on some occasions and in some subjects! One can also agree that 'the series of certain subjects may recur almost exactly without change' and that 'these do not occur very often'. To this reviewer's eye, the most striking feature of the data is the enormous variability (especially between subjects) in the shape and level of CFFSE curves. And some of the individual values are incredibly high. Remembering that CFFs as high as 100 c/s have seldom been reported it is disconcerting to find Ákos and Ákos reporting CFFs of 600–11,030 c/s, with never a critical comment or mention of possible faults in their apparatus and psychophysical technique.

There may well be a 'series effect' of central origin in the CFF, but experimentation of an altogether more rigorous and critical kind than that reported in this monograph is needed to demonstrate it. The price of this book is not stated on its jacket but, however little it may cost, it cannot be recommended as a serious contribution to knowledge of either flicker or the brain.

G. W. GRANGER

Attitude Formation and Change. By J. D. HALLORAN. Leicester University Press. 1967. Pp. 167. 21*s*.

This book is Working Paper No. 2 of the Television Research Committee, and its aim was to survey 'the fruits of previous research on attitude formation and change, placing it in the context of the Committee's terms of reference', i.e. principally 'to investigate the extent to which the developing techniques of research of the social sciences can be used to examine the influence of television on the formation or altering of attitudes and moral concepts'. It is intended to be 'of interest to students and professionals in the field...and...also to the many laymen who take these issues seriously'.

It sets out to achieve this aim mainly by means of four chapters and a conclusion summarizing the views of various people writing during the last fifteen years on the nature, formation and function of attitudes, and on attitude change. This is followed by two appendices, one on moral development, similar to the previous chapters but with a slightly longer historical perspective, and the other, contributed by Marie Jahoda, outlining the essential elements of procedure in social research.

The chapter on the nature of attitudes is a comparison of definitions of attitudes, in terms of their differentiation from closely related concepts such as belief, opinion, and value, and their internal differentiation into component aspects such as cognitive, affective, and 'action tendency'. There is no attempt to judge the operational merits of the definitions reported, or to discuss the underlying logic of statements about dispositions.

The chapter on the formation of attitudes deals with the socialization processes, including the formation of ego-defences and relations with reference groups; and the chapter on the function of attitudes discusses principally two formulations of the relationship between attitude and person-

Experience Structure and Adaptability. Edited by O. J. Harvey. New York: Springer. 1966. Pp. viii + 406. $9.00.

The main aim of this volume, which results from a conference held in Boulder, U.S.A., in 1964, is to examine the effect of early experience on the ability of individuals to behave creatively and to deal successfully with changing and complex environments. Despite the efforts of the editor, the book emerges as a collection of sixteen contributions on a variety of topics, some of which are more relevant to the stated theme than others. As the subjects considered range from creativity in artists to the use of hypnosis in inducing specially designed past histories, and from international relations to autotelic responsive environments for exceptional children, it is safe to say that few psychologists will fail to find something of interest.

The contributions are divided into seven sections, the first of which is devoted to a single chapter by Harvey on the rigidity of social systems. In the second section there is a chapter by James Bieri, who pleads for more attention to be given to the conceptual analysis of *stimuli* as well as responses in the study of cognitive processes, and suggests ways in which we might investigate how the child learns to deal with increasingly complex stimuli in his social environment; and another contribution by Harvey discussing the linkage between levels of complexity of cognitive function, personality traits and methods of training. In Part III, William Thompson attempts to relate flexibility and rigidity in its various aspects to genetic and developmental factors, and Sheldon White describes some interesting experiments on discrimination learning in young children. Part IV contains a comprehensive review by Victor Denenburg of recent work on factors influencing emotionality in rats; a well-written summary by Mary Ainsworth of the literature on the effects of maternal deprivation on intellectual development; a clear and detailed account by Omar Khayyam Moore of the working of a laboratory using the 'talking typewriter' technique, which, it is claimed, is suitable for both slow learners and gifted children; and a description by Darrel Adams and colleagues of their novel attempts to blot out, by means of hypnosis, the actual pasts of individuals and to implant substitute developmental histories, in order to explore the connexion between these and conceptual systems.

In Part V, J. McVicker Hunt, in an excellent chapter, discusses the ways in which our psychological tenets have changed in recent decades and how new evidence lends support to the value of enriching the experience of culturally deprived children at the preschool stage, and David Hunt outlines a theory of conceptual development and its application to education. Part VI explores less familiar territory, providing contributions by J. Morton Briggs on social influences on the scientist and his work, and by Richard Brody on conceptual schemes in international relations. The last section contains a chapter by Getzels and Czikszentmihalyi, who stress the difficulty of arriving at suitable criteria for the study of creativity, and William Scott ends the book with a critical examination of the concepts of flexibility, rigidity and adaptation.

The variety of the contents of the volume and of the approaches of the writers make it impossible to appraise the book as a whole or to state who would most profit from reading it. Some of the chapters are entirely clear and would be useful as an introduction to the field of study under consideration; others are somewhat obscure and ponderous in style, and not easy to follow without a considerable background of knowledge. Nevertheless, it can be said that the writing is generally of a high standard, and there is much that is stimulating, thought-provoking and challenging in the book. Many of the topics considered have been relatively neglected and yet are of great importance to psychologists, and it is refreshing to find them dealt with largely from the developmental point of view. Many ideas for further research work are suggested, and the bibliographies provided at the end of each chapter are extremely useful. MAURICE CHAZAN

The Critical Flicker Frequency Series Effect. By Károly Ákos and Magda Ákos. Budapest: Akadémiai Kiadó. 1966. Pp. 245. $7.50.

The authors of this monograph claim to have discovered a systematic variation in CFF data which has gone unnoticed by other workers. This CFF 'series effect' (CFFSE) is supposed to reveal itself when a series of fifty or so CFF measurements are made in rapid succession. Ákos and Ákos consider that CFFSE results from 'stress' induced in the visual cortex by repeated CFF determination.

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11,000 Seven Year Olds. By KELLMER PRINGLE. London: Longmans. 1967. Pp. xx + 246. 21*s*.

This is the first report of the National Child Development Study, a longitudinal survey of a large group of boys and girls, all born in one week in March 1958. This report was specially produced at the request of the Plowden Committee and is published in an almost complete version as Appendix X in their report 'Children and Their Primary Schools'. The study was initiated to review the administration of the British Maternity Services and the causes of perinatal death. The first contact with the mother and the midwife, or doctor, who delivered her provided a vast amount of medical and sociological data, including details of parents' education and occupation, antenatal care, obstetrical complications, duration of pregnancy and the condition and weight of the baby at birth.

The study then lay fallow for a number of years, until the children were traced in the last year of their infant schools.

The second contact—or series of contacts, for information about each child comes from several sources—was once again extremely broad and comprehensive. An educational assessment book (including five tests as well as teachers' comments and ratings) was completed by teachers and children and there were also parental and medical questionnaires. The testing and interviewing were carried out by personnel employed by the local authorities. The short-term aims of this project were:

'(*a*) To study the educational, behavioural, emotional, social and physical development of a large and representative group of British children in order to gather normative data; to investigate the complex interrelationships between the many facets, both normal and deviant, of children's development; and to report the incidence of handicaps and the provision currently being made.

(*b*) To utilize the uniquely comprehensive perinatal data already available in an evaluation of the relationships between conditions during pregnancy and at birth, both medical and social, and the development of children in all its aspects at the age of seven years. From this investigation it is hoped amongst other things to determine some of the factors at birth which place children "at risk" of developing handicapping conditions. Such information should permit early identification of "vulnerable" children so that earlier diagnosis and treatment, or provision, will be possible. The kinds of disability about which this sort of information is needed are not only the grosser forms, but also the more numerous "minimal" handicaps which, if undetected, at best prevent children from realizing their full potential, and at worst cause grave psychological damage due to the covert nature of the difficulties.'

While applauding the speed with which this report was produced—the sample was traced after an interval of over six years, the material collected and written up all within a period of 19 months—one is also aware of the restriction that this haste has imposed. Only the returns received by mid August 1965 referring to English pupils were included. Nearly 11,000 educational booklets were returned by that date, and nearly 8 000 maternal and medical questionnaires, but when the information from these two fields is interrelated the number falls to just below 7 000. We are not told how many children remained untraced but there are tables to show that the late returns referring to a further 1 878 boys and girls are significantly different from the group that formed the basis of this analysis in terms of social class, level of adjustment and reading attainment. This means that the instance of poor conditions and progress is probably under-reported.

The descriptive material presented covers many fields, but the section on the interrelationships of various factors, potentially the most interesting for the general reader, is disappointingly thin.

Our appetite has now been whetted and we look forward to further publications linking the early perinatal material with the later school, sociological and medical information, and fuller and more thorough analyses of the associations of many of the variables. Longitudinal studies cannot provide causal explanations of the various relationships, but nevertheless a description of a piece of natural history and the overlap of many factors can be fascinating.

JEAN M. ROSS

and now brashly inflated by a hand-to-mouth empiricism into one great blooming buzzing confusion.

But if psychology is regaining consciousness, it will one day regain memory too, and perhaps even overcome its Oedipal antagonisms. Then it will remember its old home, and the philosophical parent who taught it that psychology is the study of mental life and gave it a chart of the coast. But psychology decided to make its own way in the world. 'As a method...it has done some good', wrote Ward (1918) fifty years ago, 'as a pretended science in the hands of tyros whose psychological training has not even begun, it has done infinite harm'. And it took its journey into a far country, and there wasted its substance with riotous living. 'Are we not trying to do much too much, much too quickly?' writes Zangwill (1965), '...If psychology goes on as it is it will soon cease to be even the hope of a science; it will become the graveyard of a profession'. That is what it took to bring the prodigal son to his senses.

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areas: each is a complex phenomenon which cuts across the other areas. Learning and motivation are connected by, for example, the concept of reinforcement; and if perception is dependent on past experience, it must be related to both learning and motivation; and what is the difference between perception and sensation?—'their distinction as separate systematic concepts', writes Corso, 'has not been established in the scientific vocabulary'. And so we could go on. Whoever reads this book continuously cannot fail to observe that its subdivisions are shot through with common problems and over-lapping topics. These areas are not really diverse; they have only been made to seem diverse through being studied separately.

Psychology, it would seem, should be a system of related specialisms. This presupposes an analysis of the subject as a whole into its proper components, and an exposition of the relations among them. But contemporary psychology, and this book which reflects it so closely, are far from achieving this. If we were not so accustomed to the current subdivisions of the subject, we should, I think, be extremely puzzled to know what rational analysis they are supposed to embody. It is doubtful, to say the least, whether our specialisms have been adequately distinguished; and it follows that they cannot be properly related. It seems to me that the student who passes from one to another meets a wearisome muddle of related problems in different disguises. But the successive experts cannot help him to penetrate the disguises and expose the common features. Secure in their specialisms, they do not encounter the difficulty; they merely create it. Corso points to the weakness when he mentions 'the artificial boundaries which have been fostered in contemporary psychology by the arbitrary attachment of classificatory labels to psychological problems, e.g. learning, perception, motivation'. And Guilford points to the solution when he writes, 'One of the first needs in exploring any new field, is to map it in terms of a system of concepts'. An essential preliminary to a system of specialisms is an analysis of psychological concepts and the relations among them. This offers a chart of the coast, from which the specialist, like an inland explorer, can take his departure, and through which he can relate his findings to those of other specialists. This alone can provide the 'perspective needed for intelligent specialization in a more restricted field', and this we lack today. It is no accident that the present volume contains no introductory chapter setting out the scope, subdivisions, and methods of psychology in an orderly way. And no accident that so many of the contributors find it necessary to preface their specialist reports with their own accounts of what they consider to be the logic of psychological inquiry. These all need to be brought together, so that all may work to the same plan or at least know where they disagree. If the editors had considered this (perhaps they did?), they would probably have found, however, that a reasoned justification of the current classifications was impossible.

There will no doubt always be an important place in psychology for the man who likes his empiricism unfettered by systematic reflection. But we may agree with the editors when they suggest that this element in our tradition has been followed too exclusively. Indeed, I think we must go further. The present subdivisions of psychology are not, for the most part, the fruit of any agreed and deliberate analysis. They are historical flotsam. With the decline of the schools (which gave some order, however limited and controversial), we have inherited a haphazard collection of 'topics', too often reminiscent of the untutored common sense of an antique faculty theory, But the editors do not intend their book to be taken as a compendium, bit by bit. They speak of 'the goals of a general approach'. They intend the book to be studied as a whole, in order to give knowledge of contemporary psychology as a whole, and so to give 'the perspective needed for intelligent specialization in a more restricted field'. Here, I think, we must be cautious. If we ask whether this book reflects contemporary psychology accurately, in my opinion it does so exceptionally well. It is unusually well-balanced and representative. But whether such an accurate picture can give 'the perspective needed for intelligent specialization' is more doubtful. If it is to achieve this, contemporary psychology must presumably form at least an approximation to a meaningful whole, within which 'a more restricted field' finds a rational place. But does it? To congratulate the painter on catching an excellent likeness is not to compliment the sitter on possessing a beautiful face. What does this accurate picture reveal?

First, psychology today has not become the science of behaviour which Watson hoped for half a century ago. A basic task for a science of behaviour is, as Hirsch remarks, that 'the student of behaviour must select out of the continuous stream of activity which is behaviour, those units which are suitable for study and which can be classified'. But, as he goes on to say, 'As yet, no taxonomic system has been devised which attempts to do for behaviour what the Linnean system attempted to do for species'; and as Hall later writes, 'Psychologists do not have any generally accepted classification or taxonomy of behaviour'. It seems to follow inevitably that current attempts at behaviour theory will prove unsatisfactory, and this is exactly the upshot of Bitterman's obituary of learning theories. Further, the idea that 'behaviour' may be substituted for 'experience' as the scientific subject-matter of psychology often seems remote and impractical, especially in the context of so wide a survey as this. At the beginning of the first chapter, on scaling, we encounter 'psychological experience', that dreaded apparition; and we are at once involved in a distinction between 'the external stimulus' and 'the underlying experienced attribute'—a distinction which raises the problem of the relation of mind and matter. As Mace (1965) has recently remarked, 'Behaviourism, it would seem, is on the way out. Psychology is regaining consciousness'.

None of the other putative scientific schools of the twenties and thirties has fared better. Instead, psychology has exploded into a series of research areas, employing varied concepts and methods, often strongly influenced by behaviourism, but often also by psychoanalysis, by Gestalt theory, by factorial analysis, or by a hundred other trends, new and old. Has psychology then become what Ryle (1949) suggested?—'the name of a somewhat arbitrary consortium of loosely connected inquiries and techniques, a consortium which neither has, nor needs, a logically trim statement of programme. . . '. If this were true, we should be mistaken in looking for perspective, or talking of the 'goals of a general approach'. And there is an element of truth in it, that the specialisms of psychology are in fact pursued in relative isolation, as if they were only loosely connected inquiries. But a book of this kind forces the reader to see that, on the contrary, they are extremely closely and intimately connected. Lanzetta remarks that 'social perception is a most complex phenomenon which cuts across such diverse areas of psychology as learning, motivation, cognition, and systems research'. But this is plainly true of every one of these fourteen research

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CRITICAL NOTICE: CONTEMPORARY PROSPECTS IN PSYCHOLOGY

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Contemporary Approaches to Psychology. Edited by HARRY HELSON and WILLIAM BEVAN. London: Van Nostrand. 1967. Pp. xii + 596. 116*s*.

The scope of psychology today is so extensive that any attempt to depict the subject as a whole faces acute problems. It seems inevitable that it should be treated as a collection of varied, and often highly technical, special fields. But then a number of questions arise which, in the nature of the case, fall outside the scope of any specialism. How are these specialisms distinguished, and how, if at all, are they related? Does any have a valid claim to a central position? Do the various specialists have interests and aims in common to an extent which justifies us in continuing to speak of psychology as a relatively distinct entity? Those whose main interest lies in a special field may feel such questions are secondary. They certainly are not secondary to those whose task it is to introduce students to 'psychology'. And it may be suggested that they concern us all: if the special fields are, or should be, related, none can be successfully pursued in isolation; the health of each depends upon the health of all. The reader of *Contemporary Approaches to Psychology* will find that these questions become more and more insistent.

Eighteen leading American psychologists co-operate to provide an advanced survey of contemporary psychology. The result is the most able and comprehensive work of its kind in a single volume which the reviewer has seen for a long time, and one which may be strongly recommended to students of all ages. The book covers theories as well as experimental data, and so, in the words of the editors, 'occupies a place between history and systems of psychology, which tend to stress theories, and handbooks of psychology, which tend to stress empirical and experimental findings'. It is divided into fourteen main research areas. Ten areas represent long-established interests: scaling (Garner and Creelman), neuropsychology (Wilson and Wilson), animal learning (Bitterman), verbal learning (Bower), motivation (Hall), sensory processes (Corso), perception (Helson), personality (Rotter), psychopathology (Phares), and social psychology (Lanzetta and Sieber). The remaining four add a more topical flavour: behaviour genetics (Hirsch and Erlenmeyer-Kimling), engineering psychology (Adams), unusual environments (Bevan), and creativity (Guilford).

One can consider the book simply as a compendium of expert accounts of psychological specialisms. So regarded, it calls for fourteen reviewers and fourteen reviews. For my part, I was especially impressed by Guilford, for breadth and solidity; Phares, for a lively tilt at the concept of mental illness; and Bitterman, for a truly masterly guide, from Lloyd Morgan to the new empiricism, through the jungle of behaviour theory in which so many good men have disappeared.

serves to account for phenomena which otherwise would remain unaccountable and possibly ignored, it also suggests new corollaries for experimental verification, and opens up fresh fields for observation and research.

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asking what are the conditions in which it fails to arise? There seem to be three: (i) when the nerve impulses reaching the brain are extremely weak, as in deep sleep or general anaesthesia; (ii) when the nerve impulses are extremely violent, as in convulsions; and (iii) in those cases where the nerve impulses are of moderate strength, but the synaptic thresholds are so low that they readily flow from one neuron to the next, as in automatic actions, whether inborn or acquired through learning. But, he argues, in the ordinary state of waking consciousness 'a considerable proportion of the neurons will be passing through levels of excitation at which the discharge of an impulse would be problematical, such neurons being *critically poised*'. The device of a critically poised reagent is a principle regularly exploited in physical instruments constructed to function as detectors and amplifiers. He therefore infers that 'consciousness is dependent on the existence of a sufficient number of such critically poised neurons: only in such conditions are willing and perceiving possible. On the basis of this conception, he concludes, 'we can face up anew to the problems inherent in a strong dualism—an interaction of brain and mind, brain receiving from mind in willed action and transmitting to mind in conscious experience'.

This would seem to be in full accordance with the idea put forward by James (1901) in the chapter quoted by Thorpe: 'consciousness becomes intense when nerve-processes are hesitant; it increases our efficiency by loading the dice'. Eccles's account provides a detailed and concrete mechanism explaining how this might actually come about. 'The spatio-temporal activity of the cortex', he says, would seem to be 'determined by three factors: (i) the afferent input; (ii) the microstructure of the neural net (partly modified by previous experience); and (iii) the postulated field of extraneous (mental) influence'—an influence which is far too slight for any physical instrument to detect, but which is detected and amplified by the working of the brain. Thus the brain apparently functions as a two-way detector and transmitter. It is, in short, on the physiologist's own confession, 'just such a machine as a ghost might operate'.

I conclude therefore that, although the events within the living brain provide (or seem to provide) the *necessary* condition for conscious activities, they cannot constitute the *sole and sufficient* conditions. The other conditions must be sought in the dispositional properties—cognitive, affective, and conative—of the hypothetical psychical component. To forestall objections like those commonly raised by philosophical critics on behalf of a monistic simplicity, let me say that I put forward this version of the dualistic hypothesis merely as an appropriate working model within the scientific framework of the empirical psychologist and neurologist which frankly accepts such concepts as matter, energy, causation, and fields. On the higher metascientific plane I should be prepared to allow that the apparent dichotomy might be overcome by a monistic synthesis, with matter perhaps appearing as an epiphenomenon of mind rather than mind of matter. In any case an empirical duality of brain and consciousness is by no means incompatible with a metaphysical identity of matter and mind. But here such transcendental speculations are entirely irrelevant.

No doubt certain scientific critics will still protest that a psychical component is by its very nature unobservable. But science, particularly contemporary science, never hesitates to postulate an unobservable existent, if that seems to be the only way to interpret the facts. And certainly, the notion of such a component, not only lated by Mach. There is evidently nothing in the nature of such fields to explain how they could acquire a psychical aspect or engender conscious experience. The differences which produce 'patterns' within an electrical field are all quantitative; the differences which characterize consciousness are irreducibly qualitative.

Köhler, therefore, introduced the further concept of a psychical or 'phenomenal' field, which was said to be 'isomorphic' with the electrophysiological field, and maintained his belief in a hypothesis of strict psychophysical parallelism. However, the few experiments since undertaken to demonstrate the alleged congruence between the two fields thus assumed have entirely failed. If there were an electrical field, it seems clear that only a comparatively small part of it would be associated with consciousness. Accordingly, since we know practically nothing about the supposed electrophysiological field, and all the observations cited in support of a field theory are drawn from experimental and introspective studies of the phenomenal field, we may, I think, safely dismiss the former as an entirely gratuitous assumption.

If then we are to attempt anything like an intelligible account of conscious experience, we seem driven to postulate some kind of mental component interacting with the physical component that we call the brain or nervous system. Such a component would have two functions to fulfil (Burt, 1961*a*). The word consciousness, as we have seen, is used sometimes in a passive sense to denote what is experienced, and sometimes in an active sense to denote the processes of experiencing. If, with practically all neurophysiologists, we adopt a representational theory of perception and cognition, then we must assume a relatively passive conscious field of 'secondary qualities'—a 'presentational continuum', as earlier British writers used to say—extending in a private cognitive space: such a field would be peculiar to the individual, and correspond approximately, but by no means precisely, with the 'input-pattern of excitation' occurring in the sensory and perceptual areas of the cortex. And if, with most epistemologists, we regard consciousness, in the sense of active awareness, as essentially a two-term relation, then we must assume, not only a cognitive field to serve as the object of the relation, but also something very like a pure ego to serve as the subject—a witness of the field, in short, what Sherrington describes as 'an "I" that counts itself a cause'. The same subject not only perceives; it also apparently wills. If so, we must suppose that this 'active factor' interacts with the brain rather like a system of non-physical forces; in that case we may conceive this active factor also as a kind of mental field. The two conscious fields would thus have some analogy with the electrical and the magnetic fields in physics. The frank acceptance of this double hypothesis makes it far easier for the psychologist to describe and discuss the observable phenomena of mental life; and the detailed properties and structure of this hypothetical component will consequently be based on psychological considerations. But these I have discussed elsewhere (Burt, 1961*b*).

As we have seen, several of the experimental neurologists, who contributed to the 1966 symposium, appear quite prepared to accept the notion of two distinguishable components—a psychical as well as a physical. Even those who think it an improbable assumption, do not deny that it is a possible assumption. No one rejects it out of hand. If, however, we adopt a theory of psychophysical interaction, we have still to consider the crucial question—what are the special conditions in which conscious experience arises? Eccles is the only neurophysiologist who attempts an answer. He begins by at times accompanied by consciousness. Accordingly, *it would seem impossible to suppose*, as so many physiologists used to do when all this was still wrapped in mystery, *that these very ordinary chemical processes can 'generate' anything like conscious experience.* Nor is it plausible to suggest, as several recent writers have done—Brain, for example, among physiologists (ap. Smythies, 1965) and Feigl and Smert among philosophers (ap. Vesey, 1964)—that 'consciousness is merely the inner or private aspect, directly known, of what is indirectly known in its outer or public aspect as states or processes of the nerve cells in the brain'. Can one seriously imagine that in one sensory area of the cortex waves of electrical negativity passing along the fibres, or emissions of hormonal substances across the synaptic clefts, should be an 'aspect' of what we experience as green or red, and that in another cortical area they are an 'aspect' of an audible sound or a feeling of pain, while in others again they possess no conscious 'aspect' whatsoever?

(4) Since the study of the structural units which make up the brain has thrown no light on the emergence of conscious experience, the only other characteristic to which we can look is the way in which these structural units are organized. The outstanding feature of the cortex is its amazing complexity. From moment to moment a multiplicity of sensory stimuli is raining in on the individual's various receptors, thus exciting more or less orderly volleys of nerve impulses; a selection of these impulses eventually reaches the neural network of the brain, and so imposes an intricate and everchanging pattern of modifications on the system of dynamical activity which never ceases even during sleep. Since the process of conduction is electrical, it is tempting to suppose that this might set up some kind of quasi-electrical field, similar to that postulated by physicists in their electromagnetic investigations. A field theory of this sort has been propounded by several physiologists and psychologists, notably by Berger, Wertheimer, and Hebb. Berger (1940), influenced by Weiss's successful use of a field theory in embryology and his *Neue Theorie der Nervenfunktionen* (1928), at first hoped that his 'Elektroenkephalographie' might enable him to plot, at least in outline, 'an electrical field for the brain'. He quickly realized that this was impracticable, and instead proposed a somewhat fanciful hypothesis based on the notion of a field of psychic force, into which the electrical potential was converted. This psychic field, so he argued, would tend to emit radiations, and thus incidentally account for thought-transference; (Berger was a firm believer in telepathy, and considered himself to be 'a good receiver').

Wertheimer, followed later by Köhler and other Gestalt psychologists, had already suggested substituting 'an electrophysiological field theory' for the older 'switchboard theory', which assumed neuronal conduction to take place along completely insulated, and relatively isolated, circuits. This extension of the Gestalt doctrine, as its supporters have pointed out, is far more consistent with the Sherringtonian view of the brain as an 'integrative mechanism' than with the assumptions of the Pavlovian and Hullian stimulus-and-response school, who treat it as a mere 'associative mechanism'. It is clear, however, that any electrostatic field, which we might imagine to be set up by travelling changes in the electrical potentials within the neural meshwork, would still be a purely physical field. Indeed the processes of dynamical self-distribution—the characteristic that chiefly interested the Gestalt psychologists—were explicitly conceived as physical processes governed by the principles formu-

McDougall described how 'sensation seems here to flash out as a nerve impulse overcomes the intervening resistance, and leaps across from one nerve fibre to the next, like a spark between two electric terminals'. A somewhat similar view has been adopted by Place (1956) in an article in this journal, except that he apparently holds that consciousness is not caused by, but actually identical with, 'the passage of nerve impulses over the synapses'.

The precise nature of synaptic transmission has for long been a subject of much speculation and controversy. Even as late as the 1950's, several German neurologists, notably Nachmansohn, contended that it was essentially electrical. Work with the electron microscope removed all doubt about the existence of the synaptic cleft; and the crucial problem was eventually solved, when Eccles succeeded in inserting micropipettes into the pre- and post-synaptic nerve cells, and so demonstrated that the transmission is chemical. The substances involved (noradrenalin, acetylcholine, aminobutyric acid, etc.) are all of a familiar hormonal character. A few thousand molecules are collected and stored in minute mobile sacs or vesicles which can be seen in the knob-like terminals of the axon. The size of each is so minute that, as Eccles points out, their movements would seem to be subject to the uncertainty principle of quantum physics: in that case the old assumption of strict causal determination falls to the ground. Whenever one of them happens to collide with the membrane, it spills its contents into the synaptic cleft, always apparently in the same amount. When a train of action potentials reaches the terminal fibrils, it greatly speeds up this process; and the probability that the neuron will fire (or be otherwise affected) is thereby appreciably increased. In the post-synaptic membrane, it is supposed, there are minute pores or channels of two different sizes. When an excitatory cell releases a sufficient amount of its substance, this unplugs the larger channels, and permits the free inflow of sodium ions. When an inhibitory cell releases its substance, the effect is to unplug the smaller channels; these allow the passage of potassium and chloride ions only, and the result is a hyperpolarization of the post-synaptic cell instead of a depolarization. The various transmitter-substances are at once broken up by appropriate enzymes; and the detailed study of the whole process has already done much to explain the action of tranquillizers, stimulants, and the newer psychogenic drugs; while the experimental use of such drugs has in turn thrown further light on the nature of the process (cf. Bradley, Flugel & Hoch, 1964).

Once again, therefore, there appears to be nothing very extraordinary about the activities involved. Gray relates that, when he first began to examine the cortex with the electron microscope, he expected to find, in the areas concerned with higher functions, 'something special about the synaptic organization, which was absent in the spinal cord: however', he adds, 'the basic nature of the synapse seems to be constant throughout the nervous system'. This has been fully confirmed by Colonnier and others, who note 'the remarkable similarity shown by all synapses, alike in their essential structural features and in their mode of chemical transmission'.

We have now briefly reviewed what is known about the physical structure, chemical composition, and various activities of the cell body, the cell fibre, and the cell junction. In each of them and in all three respects there are, as Eccles has repeatedly emphasized, no essential differences between the parts of the nervous system which, like the spinal cord, are invariably unconscious and those which, like the cortex, are axon of the squid is nearly a millimetre in diameter; and, by probing it with microelectrodes, it is relatively easy to follow the movement in and out of it of radioactively labelled substances; similar studies have since been carried out in other animals on both medullated and non-medullated fibres. The membrane of the fibre separates the aqueous solution inside it from the aqueous solution that bathes it. The latter, a filtrate of the blood, is rich in positive sodium ions and negative chloride ions, and poor in positive potassium ions; within the fibre the proportions are reversed. Neither solution is highly electroconductive: the resistance offered by the fluid core to an electric current is nearly 100 million times greater than that of a copper wire, and the membrane is far more leaky. Obviously, therefore, what is conducted by the fibre cannot be an ordinary electrical current; and an electrical pulse, applied artificially, fades out after a few millimetres. The membrane which clothes the fibre proves to be readily permeable to the smaller potassium and chloride ions, but less permeable to the larger sodium ions. Hence, during the resting state, the inside of the fibre is negative with respect to the outside; and the difference is further maintained (and when necessary restored) by a metabolic pump within the membrane. As with other cells, any injury or stimulation increases the permeability of the membrane; positive sodium ions pass in, and the inside of the fibre at this point consequently becomes momentarily positive. The membrane is thus locally depolarized. The permeability spreads; but impermeability is almost immediately restored. The result is the familiar action current—a wave of negative electricity propagated along the fibre. As Sherrington has vividly expressed it, there is 'a fleeting, self-mending electrical leak, which travels down the skin of the nerve thread'. The refractory period of the nerve fibre sets an upper limit to the frequency of these all-or-none impulses, when they occur (as they usually do) in a series or train.

In its physical nature, therefore, the mechanism by which 'messages' and 'information' are conveyed turns out to be extremely simple. 'All the essential phenomena', as Eccles points out, 'can be reproduced by an artificial model in the laboratory, like that described by Lillie many years ago'. It is true that in the auditory nerve there appears to be a rough and limited synchronization with the periodicity of the stimulus; and the different second-order neurons seem, as it were, tuned to different frequencies. In the brain, however, as Bremer has shown, the frequency of the nerve impulses depends, not on the frequency of the stimulus, but on its intensity. But, whatever the details may be, the basic puzzle still remains: how can merely quantitative differences in the spatial or temporal pattern produce qualitative differences in sensation?

(3) Until quite recently many continental neurologists still maintained that during its early development the cytoplasm of the nerve cell literally grew into the cytoplasm of adjacent nerve cells, so that, unlike other organs, the brain and nervous system could not be regarded as consisting of individual cell units. On the other hand, the marked differences between conduction in a nerve fibre and in a reflex arc led Sherrington (1906) to postulate the existence of a gap or 'synapse' with a variable threshold, the variability being greatest in the higher levels of the nervous system. With those who accepted this novel concept, the unknown processes taking place at these hypothetical gaps were made to do all the difficult bits of work in the brain. A few enthusiasts even hailed them as the long-sought 'seats of consciousness'.

writers of the atomistic school, that specific elementary sensations were generated by specific *Empfindungszellen*, and that ideas and memories were stored in specific *Erinnerungszellen* within the cerebral cortex (cf. Ziehen, 1924). One still finds much the same occult functions attributed to 'brain cells' even by contemporary British and American writers, especially in their more popular expositions. The highly refined techniques developed during the last few decades have now removed a good deal of the ignorance and mystery that formerly invested the 'brain cell'. The main results have been systematically reviewed in Hydén's monograph, *The Neuron* (1961).

It turns out that, both in general structure and in chemical composition, the neuron is surprisingly similar to all other cells; but, like those composing every specialized organ or tissue, it has become highly differentiated, so as to fulfil its own particular task. Most cytologists seem now agreed that the functions of what older writers called the nerve cell, i.e. the cell body, are limited to those of growth, nutrition, regeneration, and the production of energy. Cell bodies vary widely in size and shape; but, as Sholl and others have pointed out, the variations seem chiefly related to the 'cell territory' and to its 'mode of connectivity', particularly the thickness and length of its axon. The metabolic processes of the nerve cell resemble those of other living cells. There is no specific kind of neural energy, such as McDougall and others postulated. The energy required for neural activity is obtained by the same processes as are observable in all aerobic heterotrophic cells (a liver cell, for example): it is derived from glucose, which, by oxidation and phosphorylation within the mitochondria, is progressively converted into adenosine triphosphate, whose chemical bonds form the common carrier of energy in the cells of both plants and animals. The neuron is in fact just a gland-like cell with a rather high rate of synthesizing proteins, lipids, and ribonucleic acid—a rate that is demonstrably increased with neural activity. Hydén has succeeded in dissecting out isolated nerve-cells, and so studying the incidental changes in their chemical composition. After neural activity, he finds there is not only an increase in RNA, but often a difference in the proportions of the bases (especially of adenin to uracil). The consequence, he thinks, might be to alter either the nature or the amount of the transmitter-substance, and so account for the phenomena of learning and memory. With the analogy of the genetic code in mind, he goes on to propound a molecular theory for the storage of information, effected by code-like modifications in the sequence of the constituent bases. As we have already seen, this ingenious speculation now receives but little support.

(2) One of the earliest and most stubborn problems for the neurophysiologist arises from the qualitative differences between sensations, even within the same modality (e.g. differences in colour or pitch). Helmholtz propounded a theory of 'specific nerve fibres' with different types of energy; but Du Bois-Raymond showed that the energy which all of them conduct is the same. Freud (1895) in a long-forgotten paper put forward a supplementary hypothesis, based on his experiments on the crayfish, maintaining that these qualitative differences were determined by differences in the periodicity of the neuronal currents—a notion similar to that advanced by several later theorists (cf. Fessard, ap. Delafresnaye, 1954).

During the last ten years or so the detailed nature of nerve conduction has been greatly elucidated by the biochemical researches of Francis Huxley & Hodgkin (1958) and Katz (1966) in this country, as well as by Eccles in Australia. The giant following Penfield, he would rather take 'individual independence' as the best criterion. In any case, he asks, is not 'the attempt to escape from the dualism of brain and mind, by regarding it as due to different types of observation, only a shift of the problem—equivalent to substituting a dualism between incongruent forms of knowledge for the conventional dualism which posed the original question? 'epiphenomenalism is the least reasonable hypothesis'. Eccles likewise maintains that the points made in his previous paper 'contradict epiphenomenology; experience is the primary reality, all else—brains and behaviour—are derivative interpretations'.

The final 'Summation' was entrusted to Prof. Teuber. As a psychologist, he takes the four main types of conscious activity—sensory perception, coordinated movement, learning and retention, and sheer awareness, and briefly comments on the evidence or lack of evidence on the problem of their neural correlates. In each case he asks 'where do our opinions seem to converge, where do they diverge, and where are there still very obvious gaps?'. In regard to perception 'there appears an impressive agreement that a major step forward has now been made'; in particular he notes how the findings of so many experimentalists have confirmed the recent results of Hubel and Wiesel (1962) in the visual field. The chief divergences arise first in the work on the somatic-sensory system and secondly in the attempts to find analogues for a similar kind of stimulus-processing in the auditory field. The major gap consists in our ignorance about what intervenes between processes in the receptor-field and the eventual outcome in co-ordinated behaviour. As regards action he thinks that 'the convergence lies in our agreement that the motor cortex still hides rather than reveals the answer; our divergence concerns the ways we hope to bridge the gap between sensation and action; and our biggest lacuna lies in the problem of voluntary movement'. As for memory 'we agree that we do not know how the nervous system remembers a single thing, and we hold divergent views about the best strategy for finding a neural basis for rentention'. Finally, the problem of awareness marks the greatest gap of all. Here 'the most obvious region of convergence is to be found in the fact that a group of neuro-physiologists should now be willing to discuss the topic: only ten or fifteen years ago, few would have thought it a question for serious exploration'.

III. CONCLUSIONS

As Prof. Teuber has approached his survey from the psychological standpoint, may I briefly attempt it from the neurological standpoint? Here too there are four distinguishable features calling for consideration—the cell body, the cell processes, the cell junction, and the complex neural network built up out of all three. Let us therefore ask once again the question raised by McDougall (1911): 'what is there in the nature or the activities of these various structures to suggest that they can in some way produce or generate the unique phenomena of consciousness?' The answer offered by Sir Frederick Mott, one of the foremost neurologists of that day and a convinced materialist, was 'almost certainly their unique physicochemical nature and processes, the details of which modern physiology is on the brink of discovering'. What then has modern physiology succeeded in discovering that can now help to confirm or refute this familiar reply?

(1) Until a few decades ago it was still widely assumed, particularly by continental

present in the light of past experience. Conscious control emerges when unconscious methods of control are no longer adequate, as, for example, in learning complex and flexible responses rather than mere automatic reactions such as are involved in conditioning. Consciousness, therefore, seems to be 'a process essential for the effective functioning of neural machinery when it has attained an excessive degree of elaboration': and Thorpe quotes the theory, briefly developed by James (1901) as an alternative to the automaton theory, namely, that 'conscious action, by its selective emphasis, makes amends for the indeterminateness of brain action; and its distribution shows it to be just what might be expected in an organ added for the sake of steering a nervous system grown too complex to regulate itself'.

In a short paper, entitled 'Consciousness', Lord Adrian observes that those who are so firmly agreed in rejecting dualism, show no similar agreement over what is to be accepted in its place. Admittedly in our 'public' behaviour there is little or nothing which cannot be brought within the framework of physical science; and to this extent the behaviourist's hypothesis seems adequate. 'Yet for many of us there is still one thing which seems to lie outside this tidy and familiar framework—the 'I' who does the perceiving, the thinking, and the acting'.

The last paper of all, by Prof. Schaefer of Heidelberg, represents the opposite point of view, and thus, with the 'Final Discussion' which follows, brings the whole issue to a head. 'The conference', so he reminds us, 'deals with the linkage of two items of a different character, the material brain and the conscious mind'. 'Consciousness' he defines as 'what is given in immediate experience'. 'Conscious phenomena are mental; and from these definitions a hypothetical agent may be deduced which carries these mental phenomena, much as the agent 'matter' carries the physical phenomena, namely, a mind'. However, in the brain (as he shows by detailed instances) 'the same inputs may lead to both conscious and unconscious effects, and the same outputs arise from both conscious and unconscious sources'. This, he thinks, suggests that 'consciousness cannot but be an epiphenomenon—a side-effect of bodily processes'. His own conclusion is that 'man cannot be divided into a somatic and a psychic half; mind and body are abstractions resulting from methodologically different observations'. Hence the only possible answer to the problem is that 'mind and body vary in parallel, or, better said, in correspondence'.

This formulation provoked considerable criticism. Thorpe observed that the contention that all mental processes have neuronal correlates is a theory, often useful methodologically, but devoid of proof and seemingly contradicted by the arguments put forward by earlier contributors. Mackay agreed, maintaining that Schaefer's reduction had a flavour of 'nothing-buttery'. Others objected to a definition of consciousness which restricted it to what *is* experienced as distinct from the *process* of experiencing. Penfield cited the example of the patient who, when stimulated, is retrocognitively aware of a hallucinatory perception which she recognized as occurring 20 years ago, and at the same time is fully aware that she is lying on the operation table. Here what is 'given in experience' is 'two simultaneous streams of consciousness which she appreciates and relates'. He infers that there must be 'a something more that is able to see, hear, and compare—an independent personality or self'. Gomes agrees, and thinks Schaefer's evidence merely disproves the common assumption that sheer complexity of behaviour is a safe criterion of consciousness;

to 'twice two?' or 'twice three?'. In ordinary life, however, the peculiarities elicited by the artificial tests were not discernible. There were 'no complaints about split consciousness'; and, when both hands and both fields of vision were free to play their part, the patient gave no sign of feeling they were separately experienced or controlled.

Sperry himself is convinced that 'the surgery has left these people with two separate minds—two separate spheres of consciousness': (oddly enough, this was the result predicted long ago by Fechner in expounding his double-aspect theory). Sperry, however, does not believe that the results imply that consciousness has no 'real operational value'. On the contrary, he holds that, in processes involving attention and learning, 'the majority of the evidence favours the view that the effect of consciousness is critical', and that consciousness itself is certainly 'more than merely a by-product, epiphenomenon, or meta-physical parallel of the objective process'. Elsewhere he describes how he has tested a boy of 9, who lacked a corpus callosum: 'as in some cases in the medical literature, he showed almost none of the impairments observed in the adult patient'. He also notes that 'an animal with a split brain sometimes behaves as if the two hemispheres were still in direct communication'.

McDougall (1911), it may be remembered, thought that the surgical severance of the corpus callosum would provide a crucial test to show whether (as he believed) 'the conscious unity of the personality is wholly independent of the spatial and functional unity of the brain'. However, in the discussion that followed Sperry's paper, Bremer argued that the unity of consciousness might still be mediated by long looplines reaching from the cortex of one hemisphere down to the reticular system and back to the other cortex. Eccles replied that this would increase the reaction time, and suggested experiments to test the delay. MacKay, Gomes, and Phillips, all challenged the notion of two separate streams of consciousness. Phillips thought it more natural to suppose that, when the non-dominant hemisphere was separately tested, it behaved merely like an automatic computer which had learnt to read a few words and react appropriately. MacKay summed up the position by saying: 'I don't think it makes sense to attribute consciousness to cerebral hemispheres: it is the *person* who is conscious; there is only one person that is conscious, but he has a split control system, and can sometimes pay attention to one half at a time, in a way we cannot because of the commissural coupling'.

Two other British scientists, both from Cambridge, contribute papers. Prof. Thorpe's might fairly be described as a succinct and lucid summary of the views he has since set forth in his book on *Science, Man, and Morals* (1965). Evidence, he says, analogous to that by which we justify our conviction that other human beings are conscious (though of course far more meagre) can be discerned far down in the animal scale: 'even among the lower metazoa there are species that show signs of attending selectively'. Moreover, 'consciousness appears to have evolved more than once—probably a number of times, in different animal lineages'. In vertebrates the increasing influx of sensory stimulation calls for 'physiological filtering mechanisms' at various levels within the nervous system in order to fulfil the biological need for selectivity in critical emergencies. In the course of evolution consciousness manifests itself progressively by an increase in the capacity for sustained attention, for primitive foresight, anticipation and expectancy, and the interpretation of the perience or be comparatively new'. The effects could often be changed by altering the associations or the mental set of the patient prior to stimulation. Penfield also mentions changes in the response obtained on returning to the same point on the cortex, especially after a slight delay.

Penfield's results have been cited by Levy and others (1961) as offering 'an experimental demonstration that consciousness is generated by the brain—the view of the epiphenomenalist rather than the dualist'. But no such claim is warranted by the actual facts. The most that they suggest is that the brain may co-operate in generating what is sometimes called 'passive consciousness'—the sensory and perceptual contents of consciousness—or at least in determining their nature; they in no way imply that it is responsible for the phenomena of 'active consciousness'. As Penfield remarks, electrical stimulation never elicits such experiences as 'voluntary attention', or anything resembling the more active processes of deciding, choosing, or carrying out a skilled action. He himself therefore remains a dualist rather than an epiphenomenalist. 'It is', he agrees, 'hard to conceive that our being should consist of two separate elements—body and mind; but it is equally incomprehensible that there should be only one element presenting itself as two'.

Sperry reports the effects of 'brain bisection' on the conscious experience of two epileptics. These in the main confirm the results of his 'split brain' experiments on cats and monkeys (Sperry, 1964). The operation involved a complete severance of the corpus callosum and of the anterior and hippocampal commissures, as well as in one patient the thalamic massa intermedia: (in the other this appeared to be already absent). In both there was at first some difficulty in short-term memory and in co-ordinating movements that required co-operation between the two hands. The first patient, a man of 49 and of more than average intelligence, 'was left without any gross change in personality or level of intelligence'. Minor limitations were revealed by tests; but these may have been partly due to an earlier brain injury. With the second patient 'recovery was even more complete'. By the end of the seventh month either hand could apparently be controlled by either hemisphere; four weeks later she was cooking, shopping, running her household, and driving a car. In fact 'her overall condition is now amazingly normal'.

However, certain impairments still remain, though these, we are told, 'can be detected only by special testing'. Speech, for example, is apparently possible only with the aid of the left or dominant hemisphere. When the patient was blindfolded, and a coin, pencil, or toothbrush was placed in her left hand, the right hemisphere seemed to know what it was; but the left hemisphere was ignorant of what was going on unless the right hand also felt the object, when the patient could at once name it. What is especially surprising is that the initial alexia in the left half of the visual field soon disappeared. Thus, if a word like 'cup', 'fork', or 'apple' was flashed on a screen to the left of the visual field so that only the right or speechless hemisphere could perceive it, the left hand could point to the corresponding object. If it was flashed on the screen to the right of the visual field, the right hand (as one might expect) could point to the object indicated, but the left hand could not, until the left hemisphere had named it aloud, and so told the other hemisphere what the word was. Generally speaking, the mental abilities of the right (non-dominant) hemisphere appeared to be severely limited: it could not, for example, indicate the right answer often and so surprisingly survive after extensive brain damage. Long-term memory, he argues, must depend on structural changes which are essentially synaptic; and he inclines to the view that the increased efficacy that results from a repeated channelling of the same neural pathways may be explained by a growth and multiplication of secondary excitatory synapses. This seems to be supported by recent work with the electron microscope. The larger neurons of the neocortex and the hippocampus show an exceptional wealth of dendrites, and these are exceptionally rich in receptive synaptic surfaces. Their number is extremely variable from one individual brain to another; and it seems plausible to suppose that their multiplication might be an effect of previous inter-neuronal activity (cf. Hamlyn, 1963). Eccles accepts the evidence of Hydén (1961) that there is 'an increase in the RNA content of the neurons subjected to repeated stimulation'. This extra RNA, he suggests, might itself be responsible for the protein synthesis needed for the growth of these additional contacts. But he entirely rejects 'the highly specific chemical phenomena postulated by Hydén's molecular theory of learning'; and most of the other neurophysiologists seem inclined to agree.

All the contributors apparently endorse Fessard's (1961) working assumption that 'for every conscious experience there is a specific spatio-temporal pattern of activity, but not necessarily a specific spatial structure'. Moruzzi, in a paper summarizing his well known researches on the physiology of sleep, concludes that 'only an extremely small proportion of the ongoing neural activity is associated with consciousness, though within limits we may apparently direct our attention to other neuronal patterns, which then give rise to actual experience'. Libet and Jasper describe the effects of applying very weak electrical pulses to the exposed cortex of conscious and anaesthetized patients with a view to determining the minimal stimulus required for a conscious experience. In the conscious subject there are small after-waves; and a delay of at least half a second always intervenes before any sensation is experienced. This would correspond to a relay of about 200 synapses before the necessary activity occurs which gives rise to the actual experience. All the investigators report frequent changes or displacements in the responses observed on stimulating the same point again and again.

Penfield briefly reviews the main results obtained in his well known studies of patients subjected to craniotomy under local anaesthesia. The number of these surgical cases, he says, now amounts to well over a thousand; and practically every accessible part of the brain's surface has been explored. In his present paper he is concerned more especially with the relatively non-specific areas of the human cortex. 'Because of inborn connexions the sensory and motor areas of man's cortex appear to be committed as to function: not so the new or uncommitted cortex between the auditory and the visual areas'. This portion only becomes progressively organized during the first decade of life, being usually devoted to speech in the left hemisphere and to perception (especially spatial orientation) in the right hemisphere. Penfield's observations on the temporal lobe have recently been extended by Delgado and his coworkers at Yale, who have implanted electrodes in the brains of volunteer patients. Instead of a definite playback or re-enactment of some previous experience, which Penfield's account suggests, the stimulation 'seems to favour hallucinatory, *déjà vu*. or dream-like experiences, which may either incorporate elements from past ex-

II. THE EVIDENCE

In his opening address Eccles begins by explaining the main purpose of the symposium, namely, 'to relate psychology to what we may call the neurosciences'. In the past, he says, there have been debates and conferences on specialized problems falling under this head, such as the part played by the brain in the psychological activities of perception, conditioning, information-processing, or the like; here the issue is at once more general and more fundamental—'the relation of Brain and Conscious Experience, the greatest scientific and philosophic problem confronting man'. He registers a sharp protest against those 'obscurantist philosophers', who would foreclose the whole question by declaring it to be no more than 'a pseudo-problem arising from a so-called category-mistake', and notes that one or two British philosophers and psychologists have already 'reacted against such dogmatic strictures'. But the programme of the conference, he insists, has been deliberately planned so as to be strictly physiological in its orientation.

The first paper, by Prof. Colonnier of Montreal, is anatomical, and, appropriately enough, presents a general review of the 'structural design of the cortex'. It supplements Sholl's well known survey (*The Organization of the Cerebral Cortex*, 1956) by results since obtained with the electron microscope. Sholl's main conclusion was that 'the brain operates on a probability basis'; and at times he even seemed to accept something like Thomson's 'random sampling theory of mental activity'. Colonnier, however, thinks he underestimated the importance of the columnar organization of the cortex and the selectivity of the axons, particularly in the sensory regions, and consequently overemphasized the random element. Admittedly much of the interweaving network that makes up the human brain appears to be relatively non-specific; but the areas that are closest to the input and output channels of the spinal cord are correspondingly specific. This seems to agree with what has been inferred from factor analysis, namely, that 'the brain or central nervous system has a hierarchical structure, the lower levels being the most specific, the higher the most general' (Burt, 1961*a*).

Colonnier's paper is followed by one from Eccles on 'cerebral synaptic mechanisms'. Like Colonnier, he confirms the view advanced by Gray (1959, 1966) that there are in the cortex two main types of synapse: (i) the 'axodendritic', in which the knobs or bulbs from the branches of axons are applied to spine-like projections on the dendrites of adjacent cells, and (ii) the 'axosomatic', in which they are applied to the cell body. The former, he believes, are probably excitatory; and the latter often, though perhaps not always, inhibitory. With the electron microscope synapses can now be identified by the thickening of the apposed membranes and the presence of vesicles in the presynaptic cytoplasm. With these criteria, the number of synaptic connexions made by, and with, each neuron proves to be very much larger than was formerly assumed. 'One is thus introduced to the concept of a complex wave-front comprising a multi-lane traffic in hundreds of neuronal channels, sweeping over at least 100,000 nerve cells in one second'.

Eccles' second paper deals with 'conscious experience and memory'. As a result of the great complexity of neuronal organizations in the brain, memories and habits may have a widespread and multiple representation. This would explain how they so empirical knowledge'. Much the same line is adopted by Brain. 'McDougall', he writes, 'could have known little more about the physiological aspect of the problem than an educated layman'. In this sweeping form the criticism is quite untrue. To take but a single example, the ingenious experiments carried out by McDougall and Sherrington on binocular fusion and the functions of the two hemispheres, together with the inferences they drew (whether valid or not), would never have occurred to the 'educated layman'. Almost every argument advanced by Brain is discussed in the pages of *Body and Mind*; and the few factual details that he cites were either known to or guessed by McDougall.

Nevertheless, during the past ten or fifteen years a vast amount of fresh physiological evidence has been accumulated; and the views of neurologists have themselves undergone a change almost as surprising as that of the modern physicist. Hence it seems high time that psychologists should take a new look at the results achieved. As so often happens, the remarkable advances thus obtained have been the outcome of the many new techniques which have become available owing to the rapid progress made in other scientific fields. The electron microscope now yields a magnification several hundred times greater than that of the optical instrument. The use of microelectrodes and micropipettes has made it possible to investigate the composition and reactions of single cells and fibres. Novel methods of chemical analysis allow the investigator to identify minute amounts of complex chemical substances, and the synthesis of new drugs has further helped to elucidate the problems of brain metabolism. The progress of molecular biology has contributed fresh facts and novel hypotheses. And finally, the study of communication, coding, and control in electronic automata, and the construction of working models of the brain, have suggested new and fruitful modes of interpretation and research.

Owing to the wide differences in the techniques employed, the results obtained are scattered in a variety of journals, printed in different countries and often in different languages. Fortunately, however, a few years ago a group of neurophysiologists decided that the time had arrived to organize an international conference specifically to re-examine the problem of body and mind in the context of these new investigations and discoveries. The papers read and the debates that followed have now been published (with minor additions and revisions) in a report edited by Sir John Eccles (1966) and dedicated to the memory of Sherrington and Schrödinger. The contributors record the latest conclusions reached with each of the different methods enumerated above; and the 'Final Discussion' presents an instructive survey of the present position as seen by contemporary neurophysiologists. My own purpose is to inquire what light is thrown by the results reported upon the basic problems of the psychologist. I have, in several previous papers (Burt, 1961*a* and refs.) reviewed the situation as it appeared some years ago. Here my endeavours will be, so far as limitations of space permit, to bring the discussion up to date. Each of the experimentalists who contribute to the symposium has naturally taken it for granted that his hearers and his colleagues will already be familiar with recent work in his particular field. I shall therefore draw freely on other publications, and occasionally even fall back on observations of my own when I was working as Sherrington's assistant, and was privileged to take part in many of his experiments.

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BRAIN AND CONSCIOUSNESS

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The purpose of the following paper is to consider what light has been thrown on the problem of consciousness by recent researches on the brain, particularly those carried out by the many new techniques which have become available during the last fifteen years. It appears (i) that the nerve cell differs in no essential way, either in its basic structure or in its metabolic processes, from other gland-like cells, though, like all cells, it is differentiated for its specific function; (ii) that conduction in the nerve fibre is a relatively simple electro-chemical process; (iii) that the transmission of the nerve impulse across the synapse is chemical, and the transmitter substances are of a familiar hormonal character; and (iv) that, apart from the greater complexity and the greater instability of the synaptic thresholds, there are no essential differences between those parts of the neuronal network (e.g. the cortex) which are accompanied by consciousness and those parts (e.g. the spinal cord) which are not. A comparison of the specific micro-neural situations in which consciousness does and does not arise suggests that the brain functions, not as a generator of consciousness, but rather as a two-way transmitter and detector; i.e. although its activity is apparently a necessary condition, it cannot be a sufficient condition, of conscious experience.

I. The Problem

Psychologists during the last fifty years have shown an increasing reluctance to discuss the elusive problem of the relation between body and mind. For the behaviourist, who regards the very concepts of consciousness and mind as meaningless relics of an obsolete metaphysics, the issue cannot arise; and most scientists hesitate to take up awkward questions before they can see in their mind's eye at least the rudiments of a plausible answer. Nevertheless, among physiologists, philosophers, and physicists there has of late been an unexpected revival of interest in what Schrödinger has called 'the most important problem that science has yet to face'. Three years ago a series of 'readings' was published under the title of *Body and Mind* (Vesey, 1964). Though mainly historical, this included extracts from contemporary authors in each of the three fields I have named; and several discussed such questions as 'can mental states be brain states?' A year later a set of papers by nine eminent writers appeared, all dealing expressly with the subject of *Brain and Mind* (Smythies, 1965). Three of the four neurologists came out in strong support of an uncompromising dualism. Yet, strange to say, the arguments advanced on either side, even by the neurologists, were essentially philosophical rather than physiological.

Lord Brain, for example, rejects both current materialism and oldfashioned mentalism in favour of a double aspect theory. He starts with a discussion of the interactionist hypothesis developed by McDougall in *Body and Mind* (1911). At that time the line taken by the opponents of interactionism was that 'judging by current trends, research on that mysterious unit, the nerve cell or neuron, will in the near future almost certainly reveal that the cells of the cortex and the unknown energy they transmit possess unique biochemical or physical properties, and these will quickly make it clear how sensory experience is generated by the natural activities of the waking brain: at present we are in the dark, and the interactionists' arguments in favour of an immaterial mind rest solely on physiological ignorance, not on

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prior to deprivation, but the naive subjects in the present study experienced no change in pre-test feeding.

The failure to observe an effect on over-all preferences for novelty is in line with the writer's earlier finding. However, one effect on initial reactions to the novel environment was observed, in that currently deprived rats entered the novel half of the apparatus significantly sooner than non-deprived animals. Fehrer (1956) and Bolles & de Lorge (1962*a*) have reported similar observations.

When both sexes were currently non-deprived, the higher female frequencies of activities defined as *exploration* confirm a similar sex difference reported in another study (Hughes, 1968). The sex difference in the effects of current deprivation (with *exploration* being increased for males but not affected for females) strongly resembles that reported by Thompson (1953) for locomotor activity. De Lorge & Bolles (1961) also observed an increase in male exploratory activities with food deprivation.

Over-all the results of this experiment indicate that food deprivation experience can increase locomotor activity and that the decrement in locomotion following current deprivation reported by several authors (e.g. Carr, Overall, White & Brown, 1959; Montgomery, 1953; Zimbardo & Montgomery, 1957) is only typical of such deprivationally experienced animals. Sex differences in the effects of current deprivation, although not important for locomotion alone (as reported by Thompson, 1953), would appear to be so for other measures of exploratory behaviour. Currently deprived rats will also enter a novel environment sooner than non-deprived animals. These findings suggest that the effects of food deprivation on exploratory behaviour are much more complex than some earlier investigators have perhaps assumed. To a marked extent both deprivation experience and sex are important in determining the effects of depriving rats before testing. The complexity of these effects becomes even more obvious when one considers the different relationships between deprivation and other experimental conditions that appear to exist when different indices of exploration are examined (e.g. preferences for novelty, locomotion, exploratory activities). This alone seems to justify recording more than one form of behaviour in experiments of this kind. Thus, there would not appear to be much point in trying to establish a *general* relationship between food deprivation and exploration. One can probably aim only to establish relationships between specific types of deprivation, various other experimental conditions (e.g. sex, past experience, etc.) and specific forms of behaviour, believed to serve an exploratory function. Unfortunately this complicates further the problem of the motivation of exploratory behaviour.

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Total unit entries

The results show that the deprivationally experienced, non-deprived rats engaged in significantly more locomotor activity than any of the other three groups. They also indicate that current food deprivation decreased locomotion for experienced rats but had no effect on naive rats, and that prior deprivation experience produced more locomotion when the rats were not currently food-deprived but had no effect when they were.

Exploration

Non-deprived males explored less than either non-deprived or deprived females. Also, current deprivation increased *exploration* for males, but had no effect on females. These results, plus the differences shown to contribute to the second-order interaction on this measure, reflect the tendency for female rats to explore more than males when both sexes were not food-deprived.

Although no significant main effects or interactions were observed with the *preference for novelty* measure, each rat was seen in the novel half of the apparatus at the end of more than 50 % of the 90×10 sec periods.

Because of several extremely high scores latencies of first entering the novel half were compared between conditions by non-parametric Mann–Whitney U tests. The specific comparisons were as follows:

(1) Naive (median latency = 3·5 sec) and experienced (median latency = 5·0 sec) ($U = 116{\cdot}5$, n.s.).

(2) 0 hr deprived (median latency = 9·5 sec) and 48 hr deprived (median latency = 2·0 sec) ($U = 72$; $P < 0{\cdot}05$).

(3) Males (median latency = 5·0 sec) and females (median latency = 3·5 sec) ($U = 112{\cdot}5$, n.s.).

Only the second comparison reveals a significant difference. This means that after having been replaced in the familiar half, food-deprived rats entered the novel half of the apparatus sooner than non-deprived animals.

DISCUSSION

The depressing effect of current food deprivation on the locomotor activity of deprivation-experienced subjects resembles Montgomery's (1953) finding. Bolles & de Lorge (1962*b*) reported a similar trend which, however, was not significant. In view of the significantly greater locomotor activity of non-deprived experienced rats, it would seem that their 10 days of cyclic feeding had produced a higher basal level of general activity which, in the 48 hr condition, was reduced to that of animals maintained on an *ad lib.* diet. The facilitation of locomotion in this apparatus by food deprivation earlier reported (Hughes, 1965) was not observed in the present experiment. This is not wholly surprising, since some of the conditions of the two experiments were different. For example: (i) approximately 24 hr of current deprivation were imposed in the earlier study, whereas the comparable period was approximately 48 hr in the present one; (ii) each rat was confined to the familiar half of the apparatus for about 24 hr in the earlier study but for about 12 hr in the present one; (iii) all deprived subjects in the earlier study were put on a reduced *ad lib.* diet for 3 days all proportional data (i.e. all except *total unit entries* and latency of first entering the novel half) were transformed before analysis by the formula:

$$\text{angle} = \text{arc sin}\sqrt{\text{percentage}}$$ (Snedecor, 1956; Winer, 1962).

In no case was there a significant main effect but there were several significant first- and second-order interactions, i.e. $E \times C$ for *total unit entries* ($F = 12{\cdot}27$; D.F. 1, 24; $P < 0{\cdot}01$); $C \times S$ ($F = 14{\cdot}09$; D.F. 1, 24; $P < 0{\cdot}05$) and $E \times C \times S$ for *exploration* ($F = 4{\cdot}71$; D.F. 1, 24; $P < 0{\cdot}05$); and $E \times C \times S$ for *grooming* ($F = 8{\cdot}74$; D.F. 1, 24; $P < 0{\cdot}01$).

Differences between E, C and S cell means for the two measures showing second-order interactions were tested for significance with t tests (D.F. 6). However, because of the small number of subjects per cell and high variability, very few individual comparisons revealed a significant difference. For *exploration*, experienced, non-deprived females (mean $= 74{\cdot}25$) were significantly higher than naive, non-deprived males (mean $= 53{\cdot}75$; $t = 2{\cdot}47$; $P < 0{\cdot}05$) and experienced, non-deprived males (mean $= 54{\cdot}50$; $t = 3{\cdot}81$; $P < 0{\cdot}01$). This latter group was also significantly lower than both naive, non-deprived females (mean $= 68{\cdot}75$; $t = 2{\cdot}48$; $P < 0{\cdot}05$) and naive, deprived males (mean $= 68{\cdot}00$; $t = 2{\cdot}93$; $P < 0{\cdot}05$). For *grooming*, the only significant difference was between experienced, non-deprived males (mean $= 16{\cdot}25$) and experienced, deprived females (mean $= 3{\cdot}00$; $t = 2{\cdot}48$; $P < 0{\cdot}05$).

Table 1. *Results of comparisons between individual means for interacting effects of (a) current deprivation and deprivation experience on total unit entries; (b) current deprivation and sex on exploration*

(The cells of each matrix contain t values.)

(*a*) Total unit entries

		Naive		Experienced	
		0 hr Mean = 49·89	48 hr Mean = 59·24	0 hr Mean = 83·00	48 hr Mean = 48·25
Naive	0 hr	—	0·93	3·97**	0·21
	48 hr	—	—	2·45*	1·19
Experienced	0 hr	—	—	—	4·69**
	48 hr	—	—	—	—

(*b*) Exploration

		Male		Female	
		0 hr Mean = 54·12	48 hr Mean = 69·75	0 hr Mean = 71·50	48 hr Mean = 64·50
Male	0 hr	—	3·20**	3·62**	2·14*
	48 hr	—	—	0·45	1·32
Female	0 hr	—	—	—	1·81
	48 hr	—	—	—	—

* $P < 0{\cdot}05$; ** $P < 0{\cdot}01$.

Differences between appropriate means for those measures showing first-order interactions were also tested for significance by means of t tests (D.F. 14). The comparisons were between E and C means for total unit entries and C and S means for *exploration*. These means and the t test results are displayed in Table 1.

with several pieces of wood the same size and shape as the food (48 hr deprivation group). At the end of this period the rat was removed and placed in a cage while the food cubes or pieces of wood were removed and the guillotine slides withdrawn. (Wood cubes were kept with currently deprived rats and subsequently removed to control for environmental changes resulting from the removal of material from the apparatus.) After all this (which took approximately 60 sec) the rat was put back into the same 'familiar' half and, 30 sec later, was continuously observed for 15 min. Testing took place in normal room illumination between 10 a.m. and mid-day. Water was always available in each half prior to and during testing. The novel half of a box was on the left for sixteen rats, and on the right for the other sixteen.

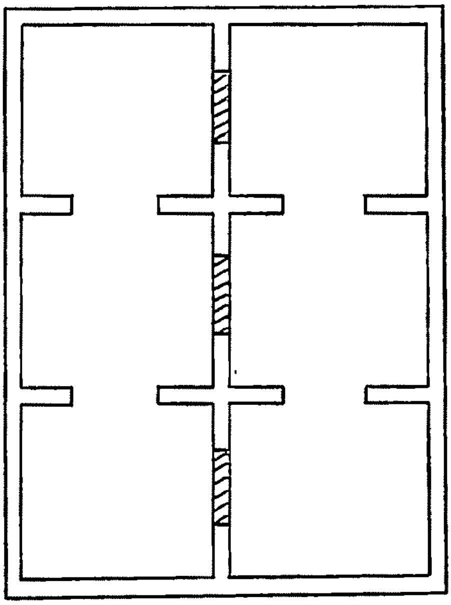

Fig. 1. Floor plan of an exploration box with guillotine slides (shaded) in place.

By means of hand-counters the total number of units entered was recorded (*total unit entries*). Also, it was noted whether the rat was in the novel half or not (*preference for novelty*), and what sort of activity it was engaged in at the instant a small light from an electric timer flashed on and off, thereby signalling the end of a 10 sec period. (*There were ninety such observations for each rat.*) The following forms of behaviour were thus recorded:

(1) *Exploration*—i.e. activities that bring a rat's receptors into closer contact with its external environment, thus possessing greater familiarization properties: (*a*) sniffing (moving the snout through the air or over the floor and walls with vibrissae twitching); (*b*) rearing (standing on hind legs unsupported or leaning against a wall while usually engaged in sniffing); (*c*) walking (moving forwards, backwards or sideways within a unit or from one unit to another).

(2) *Grooming*—licking, biting, scratching or rubbing the body.

(3) *Drinking*—drinking from a water dispenser.

(4) *Freezing*—crouching or standing on four legs entirely motionless.

In addition, the delay before the rat first entered the novel half (after having been put back into the familiar half) was recorded for each animal to the nearest second, even though all other measures were not recorded until 30 sec had elapsed.

RESULTS

Separate analyses of variance were performed on all measures, except latency of first entering the novel half. To reduce the variances and normalize the distributions

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ANALYSIS OF AUGMENTED RECALL IN SHORT-TERM MEMORY FOLLOWING A SHIFT IN CONNOTATION

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The effect of connotative history upon a critical triad of connotatively defined words was studied in short-term memory. The critical triad was presented subsequent to a build-up in proactive interference. A release from proaction was obtained only when the history was one of homogeneous items connotatively different from the critical items; heterogeneous items connotatively different from the critical items did not produce the effect. This observation replicated in a second experiment was interpreted in terms of the operation of collative variables in the condition producing the release from proactive interference.

In the analysis of short-term memory (STM) for verbal items a frequent empirical and theoretical assertion is that information at the moment of reception is sorted and classified into certain operative dimensions and held in storage with respect to these dimensions (Wickelgren, 1966; Conrad, 1967; Posner, 1966, 1967). However, the paucity of demonstrable dimensions of storage is only too apparent; as yet only the existence of an auditory code, based upon the phonemic content of the items, has been firmly established (see Posner, 1967). Attempts to show other, somewhat higher-order, codes have failed for the most part, revealing only slight evidence of an operative semantic dimension (Baddeley & Dale, 1966; Baddeley, 1966). Recently Wickens and his associates have explored various types of verbal categories in the context of a protocol which engages a shift in information categories subsequent to a build-up in proactive interference (PI) (Keppel & Underwood, 1962; Loess, 1964). In the original study, Wickens, Born & Allen (1963) generated PI through successive applications of an STM distractor technique (Murdock, 1967) employing a particular class of material (e.g. consonants); they then shifted to a new class (e.g. digits) and observed that the new information was recalled at a level approximating the recall for the original trial of the series. In later studies this phenomenon, referred to as 'release from PI', was demonstrated with shifts in connotation as defined by the 'semantic differential' (Clark & Wickens, 1966) but was not demonstrated with shifts in grammatical class (Wickens, Clark, Hill & Wittlinger, in press).

The obvious import of these data is that they are perhaps indicative of operative dimensions of classification and storage in STM. In the light of an 'acid-bath' model of STM as proposed by Posner (1966) the new information is classified and stored independently of the 'concentration' generated by the preceding information and therefore remains relatively unaffected in the storage phase. Thus from initial observation the data appear compatible with independent-storage and interference-free concepts and explanations. Yet it is noticeable that the protocol for demonstrating the release phenomenon entails that the items preceding the critical shift items are categorically homogeneous. If the release phenomenon does reflect independent dimensions of storage, then it should be manifested when the pre-critical items history is one of heterogeneous items, none of which are the same class as the critical items. If this does not obtain then it must be assumed that some alternative process underlies the phenomenon.

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The present study explores this problem with items defined according to the 'semantic differential' (Osgood, Suci & Tannenbaum, 1957).

EXPERIMENT I

Method

Subjects

The subjects were ninety students from the introductory psychology programme at the Ohio State University. Each subject was allocated at random to one of three conditions with thirty subjects per condition.

Design

The three conditions were designated on the basis of the pre-critical items history. Condition SA entailed a history of short-term storage of items from the same class as the critical items. This condition constituted the control condition. Condition O entailed a pre-critical items history of items from the class opposite to that of the critical items. Condition D involved items from different connotative classes such that (*a*) no classification was repeated in the experimental history, and (*b*) no classification was the same as that of the critical items. The design was therefore one which tested the effect of different storage histories upon the retention of a common set of items.

Material

The items were words, grouped into triads, drawn from a 1000-word sample classified according to the 'semantic differential' dimensions by Heisse (1965). A word was chosen if it was located on the extreme end of one dimension and was relatively neutral on the other two dimensions. Synonyms and antonyms of words used were prohibited. The critical triad consisted of three words classified as evaluative negative. For condition SA the words constituting the experimental history were evaluative negative; for condition O they were evaluative positive. For condition D the four triads composing the experimental history were potency positive, activity positive, evaluative positive and activity negative. These words were chosen such that, with respect to their individual profiles, none were classified on the negative end of the evaluative dimension.

Procedure

In each condition the subject received five trials. The first four trials provided the experimental history with the fifth trial being the critical trial. Each trial contained the following sequence of events presented visually by means of a Kodak Carousel projector and controlled by a tape-timer: (*a*) Ready signal of 2 sec. duration. (*b*) Word triad presented for 2 sec. and read aloud once by the subject. (*c*) Three-digit number presented throughout the retention interval from which the subject counted backwards aloud by three's to the beat of a metronome set at 60 beats per minute. This task filled the 15 sec. retention interval with the subject's performance on the task recorded by the experimenter. (*d*) Question-mark acting as recall cue for 10 sec. duration. There was no inter-trial interval as such; with the termination of the recall cue the ready signal for the next trial appeared. At recall the subject reported aloud with the experimenter registering the response.

Prior to the experiment, the experimenter read to the subject a set of instructions, common to all three conditions. An instruction sheet was provided for the subject's perusal.

Results and discussion

A decrement in recall performance from trial 1 to trial 4 obtained for all three conditions. As Fig. 1 indicates, the build-up of PI occurred at the same rate for all three conditions even though condition D involved a continual shifting of classes. An analysis of variance of the recall data for the critical triad indicated a significant effect of experimental history ($F = 3{\cdot}90$; D.F. = 2, 87; $P < 0{\cdot}05$). Dunnett's *t*-test performed on the means revealed a significant difference between condition O and condition SA ($P < 0{\cdot}01$) but no significant difference between condition D and condition SA. The over-all elevation of recall on trial 5 is probably due to a biasing produced by employing the same triad, the critical triad, across all subjects in each condition. Across the preceding four trials the triads had been counterbalanced such that each triad was tested an equal number of times at each trial position. Therefore if the critical triad was relatively easier to retain, a somewhat exaggerated recall level would be manifest with respect to trial 4.

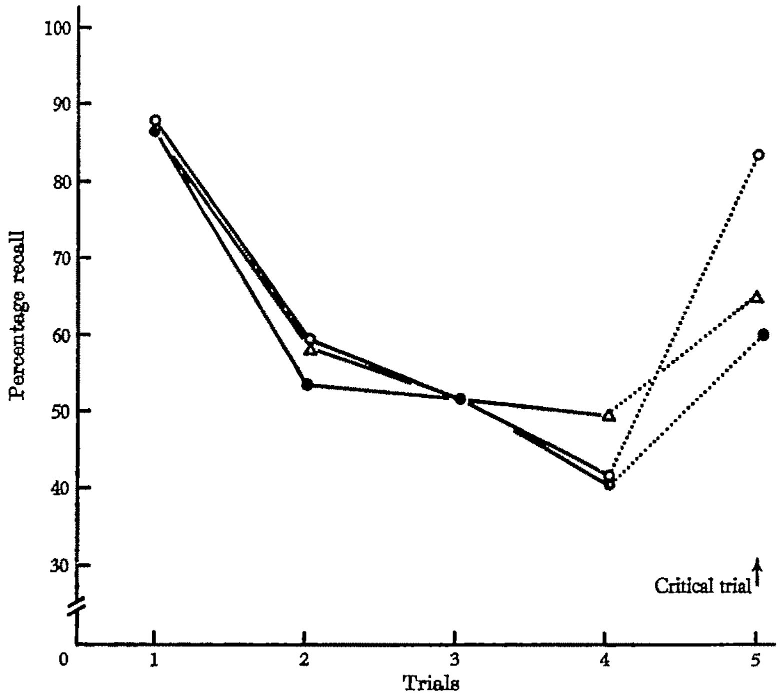

Fig. 1. Percentage recall for PI build-up trials and critical trial in Expt I. ○—○, Condition O; ●—●, condition SA; △—△, condition D.

The data therefore demonstrate that the release phenomenon is obtained when the preceding history is that of homogeneous items categorically different from the critical items, but not when the preceding history is that of heterogeneous items categorically different from the critical items. An explanation in terms of an 'acid-bath' model of forgetting does not seem feasible. Inherent in such a model is the assumption that information is sorted at input into classes for storage, similar items being stored or coded similarly. The forgetting that occurs is then a function of the time in store and the number and similarity of stored items. With respect to conditions D and O such a model would predict that the critical items should be recalled to the same degree inasmuch that in both instances the items in store are dissimilar to the critical items and the time and number variables are constant.

In contrast to this model, Peterson (1966) contends that interference occurs at recall and not in storage. The primary effect of the competition at recall that is generated by preceding items is on the guessing probability. The release phenomenon obtained in the present experiment therefore would be viewed as a function of an increased capacity to discriminate between the potential response items on the basis of connotative difference. However, in regard to the data generated under condition D, it seems that a discrimination hypothesis must predict a somewhat higher level of recall not only on trial 5 but on trials 2–4 also. A second experiment was therefore conducted to verify the observation of the present experiment and to assess the effects of a possible confounding factor in condition D.

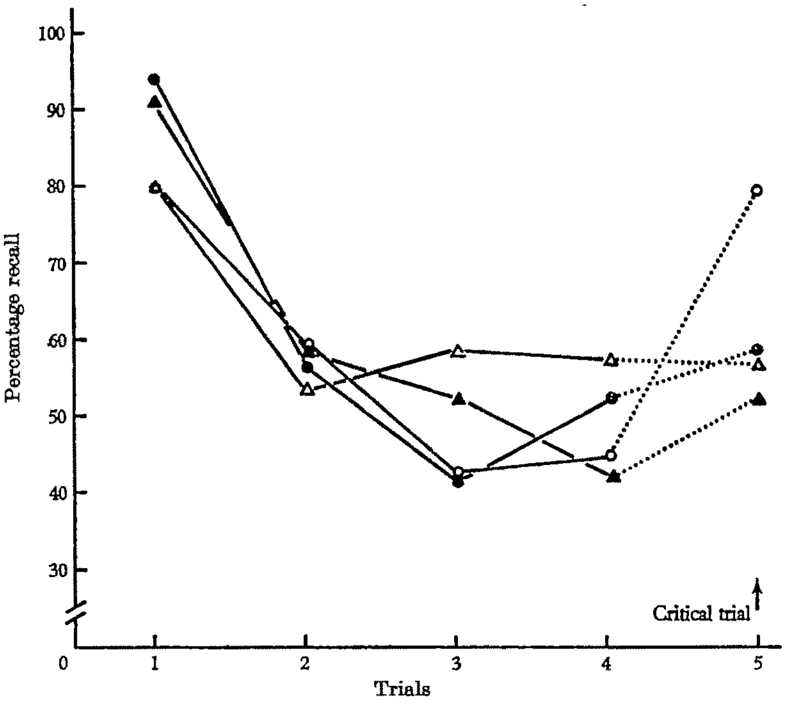

Fig. 2. Percentage recall for PI build-up trials and critical trial in Expt II. ○—○, Condition O; ●—●, condition SA; △—△, condition D; ▲—▲, condition R.

Experiment II

For this experiment a different triad of evaluative negative items was selected for the critical trial. The three conditions tested in Expt I were again studied with a fourth condition added. This condition employed the items of condition D but eliminated the intra-triad homogeneity, i.e. the items used in the experimental history phase were randomized. This was done to test the possibility that a higher-order classification (the classification of each triad as homogeneous) might be affecting the critical triad in terms of obscuring its discriminable characteristics. This condition was referred to as condition R.

For this experiment, 120 subjects were drawn from the same population as used in Expt I. The subjects were randomly allocated to each of the four conditions, again with thirty subjects to each condition. Apparatus, materials and procedure were identical to those in Expt I.

Results

The data for recall of the critical triad across conditions were subjected to an analysis of variance. The effect of conditions was significant ($F = 3{\cdot}56$; D.F. = 3, 116; $P < 0{\cdot}05$). The application of Dunnett's *t*-test indicated a significant difference between the recall means for condition O and condition SA ($P < 0{\cdot}025$) with neither of the other two conditions differing significantly from SA. As Fig. 2 indicates, the variability of the conditions across trials is somewhat greater than in Expt I. An analysis of the recall data of the first four trials showed no significant effect of conditions but a highly significant trials effect ($P < 0{\cdot}001$) and a conditions × trials effect ($P < 0{\cdot}001$). Observation of Fig. 2 shows that the recall level for condition D asymptotes somewhat sooner than the recall level for other conditions. From the assumption that switching of classes reduces PI, this perhaps should be expected. However, a similar analysis of the data from Expt I does not corroborate this observation.

Discussion

As with the previous experiment, the release phenomenon obtains only when the pre-critical triad history is one of homogeneous items different connotatively from the critical items. A history of heterogeneous items, connotatively different from the critical items, does not produce the phenomenon. The variables discussed in Posner's (1966) 'acid-bath' model are inadequate for handling these data. At first glance Peterson's (1966) argument that the release from PI represents the effects of increased discriminability and less interference at recall seems somewhat more appropriate. Yet if facilitated discrimination at recall based on the connotative differences is the underlying mechanism then one would expect to see, for example, certain differences in retention level at trial 2. For condition D the triad on trial 2 being categorically different from the preceding triad should therefore be readily discriminated and recalled at a level approximately the same as the first triad. Observation of Figs. 1 and 2 clearly shows that this is not the case. Therefore consideration of the data in the light of these two explanatory models reveals that the obtained release phenomenon does not reflect (*a*) an underlying storage process based upon connotative dimensions or (*b*) a differential effect at recall based upon connotative differences among the potential items.

With the discarding of these explanations a more trenchant analysis of the protocol generating the release phenomenon is required. First, the nature of the protocol employed is one that allows for the functioning of collative variables (Berlyne, 1960). Second, the protocol for generating the release phenomenon is homologous to the experimental paradigm for showing the von Restorff effect. Descriptive terms such as 'surprising', 'odd' and 'novel' are applicable to the appearance of the critical triad in condition O. These terms, as discussed by Berlyne (1960), are not directly applicable to the other conditions. The role of these collative variables has been assessed by a number of investigators in the production of the von Restorff effect.

Green (1956) demonstrated that a greater manifestation of the effect was obtained for the first isolated term in a series than for the second isolated term. In a later study, Green (1958) showed that it was not the isolation of the item within the series which gave rise to the effect but the structural change from the preceding homogeneous items to the critical item. This finding was corroborated by Swartz, Pronto & Engstrand (1958).

In the light of these data a descriptive interpretation in terms of the operation of collative variables may be applied to the present experiments. The manner in which these variables operate in STM and their locus of operation, i.e. whether it is at the trace formation, trace storage or trace utilization phase (Melton, 1963), cannot be discerned by an analysis of the present data. However, Berlyne (1960) has indicated that a correlate of collative variables is arousal. Recent experimentation has demonstrated that 'arousal' is an effective independent variable in STM. Maltzman, Kantor & Langdon (1966) showed that items eliciting higher GSRs were retained more effectively. They extended the hypothesis that the occurrence of an orienting response (OR) facilitates the reception of words and their retention. In this regard it must be noted that recent unpublished experiments in this laboratory have indicated that an OR elicited by an irrelevant aspect of the information display (i.e. an OR not elicited by the items to be retained) may be ineffective or even deleterious. This is similarly observed in the von Restorff effect where change itself is not a sufficient condition for the phenomenon; the change has to be within the dimension of the items being coded (Wallace, 1965). Furthermore, in a recent study Berlyne, Borsa, Hamach & Koenig (1966) demonstrated the effects of arousal upon STM but argued that the effect one obtains will be functionally dependent upon the level of arousal manifest.

It is possible to argue that the collative variables act indirectly by merely inducing greater rehearsal. This interpretation was assessed by an analysis of the present data. The retention interval performance scores generated during the storage of the critical items in conditions O and SA were subjected to a t-test. No significant difference was obtained between condition O and condition SA in either experiment, thus ruling out any interpretation stated in terms of differences in rehearsal.

In summary, the release from PI observed when there is a shift in connotative class is not indicative of dimensional storage properties of the STM mechanism. Rather the release phenomenon may be ascribed to the operation of collative variables which are contingent upon the protocol.

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CLOSURE AND RESPONSE BIAS IN SHORT-TERM MEMORY FOR FORM

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Several attempts to test the Gestalt hypothesis of autonomous change in the memory trace have shown trends which, though mutually consistent, do not fit the Gestalt hypothesis. Expt I aimed to reproduce these effects. Two figures were used (circles with gaps of 15° or 60°), one retention interval (15 sec) and two methods of testing: reproduction, in which the subject first copies the figure and then draws it from memory; and recognition, using the method of identical stimuli. In the recognition test, subjects tended to judge an identical 60° gap as 'larger' after 15 sec, implying closure of the gap in the memory trace, but the 15° gap showed no effect. No reliable changes in gap size occurred using the method of reproduction. Expt II tried to increase the sensitivity of this method by stressing the importance of reproducing the gap accurately. Again results were negative. Expt III tested the hypothesis that the recognition effect found was due to guessing habits. The initial stimulus was presented as before, but the recognition stimulus was replaced by the momentary illumination of a blank card purporting to be a brief presentation of the recognition figure. Subjects were thus forced to guess. The pattern of responses was almost identical with that found in the previous recognition study. Conclusions are: (1) the consistent trends previously found using the method of identical stimuli are probably due to guessing habits; (2) there is no evidence for autonomous change in the short-term retention of form.

In 1922 Wulf published a study which he claimed showed clear evidence of Gestalt principles in the retention of visually presented forms. Over the next 40 years, the validity of this claim was discussed in a steady stream of papers, culminating with an excellent historical and critical review of the problem by Riley (1962). In it, he points out that most of the earlier studies have subsequently been shown to be methodologically unsound, but concludes that at least two techniques can still be regarded as valid. The first of these is the method of reproduction as used by Walker & Veroff (1956), in which the subject's subsequent reproduction of a figure from memory is compared with a previous copy. The second method is that of recognition, when the recognition test is limited to a single figure. In the most elegant version of this, the recognition stimulus is identical with the original, and the subject is required to make a judgement on one crucial aspect (Irwin & Seidenfeld, 1937). Since the original and recognition stimuli are identical, any systematic change in the memory trace will be reflected in the distribution of judgements.

A survey of the studies which appear to be reasonably sound methodologically leads to the conclusion that such systematic effects as do occur, do not, on the whole, fit the Gestalt hypothesis. Partly no doubt as result of this, interest in the Gestalt hypothesis of memory has waned in recent years, and with it interest in the question of whether systematic changes occur in the retention of visual form. Closer examination of recent studies, however, suggests that certain effects occur with remarkable consistency. One of the commonest figures used in this type of study is that of a broken circle, the break being either relatively small (20° or less) or large (50° or more). Of five recent studies which have used this figure, four have used the method of identical stimuli to study short-term retention. The results of these four studies are given in Table 1.

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With the exception of the Karlin & Brennan (1957) 1 sec condition, they show a consistent picture, with small gaps tending to open and large gaps to close. The fifth study (Walker & Veroff, 1956) used the method of reproduction and compared retention tested immediately with retention after two weeks. The immediate test showed results similar to the four recognition studies: a small (20°) gap tended to open, and a large (80°) gap to close. Over the ensuing fortnight the small gap remained constant but the large gap continued to close.

Table 1. *Summary of recent studies using the method of identical stimuli*

		Small gap		Large gap	
Study	Delay (sec)	Size	Change	Size	Change
Crumbaugh (1954)	3–12	15°	Opens	90°	Closes
Brown (1956)	10	c. 10°	Opens	—	—
Karlin & Brennan (1957)	1–8	15°	Closes after 1 sec; opens after 4 and 8 sec	—	—
James (1958)	27	2°	Opens	50°	Closes

The amount of agreement shown by these five studies suggests that there is a genuine systematic effect, though not in the direction that Gestalt theory would predict. The present study aims first to establish that a replicable effect exists, and secondly to ensure that it *does* depend on memory.

The first experiment aims to reproduce the effect, using both the method of identical stimuli and the method of reproduction. If the phenomenon reflects a genuine change in the memory trace, both methods should show similar effects.

EXPERIMENT I

Method

Materials. All subjects were presented with two figures, both comprising circles of 5 cm diameter drawn in indian ink on 4½ × 3½ in. white cards. Both had a gap centred at 225°; in one the gap was 60° and in the other 15°.

Procedure. Subjects were divided at random into two equal groups. Group I (reproduction) subjects were told they would be asked to copy the figure as accurately as possible and subsequently would be asked to draw it from memory. The first stimulus was then exposed manually for 5 sec while the subject copied it on a 4½ × 3½ in. piece of paper. This was then removed, and 15 sec later the subject was given a second piece of paper and instructed to draw the figure from memory. When this was completed, he was shown the second figure and the process was repeated.

In condition II (recognition) subjects were told they would be shown a circle with a gap. They were to remember this until shown a second, similar figure. They were then to judge whether the gap in the second circle was greater or smaller than the gap in the first, or equal to it. The first stimulus was then shown for 5 sec; there was a 15 sec gap followed by the 5 sec presentation of a second, identical stimulus. The procedure was then repeated for the second pair of figures. Each member of the two identical pairs of figures served equally often as the initial and the recognition figure, and the order in which the terms 'greater', 'smaller', and 'equal' occurred in the instructions was varied at random so as to minimize the possibility of inducing a systematic response bias.

Forty young servicemen were tested individually in each condition. Half began with the large gap and half with the small.

Results and discussion

Reproduction. Subjects appeared to find accurate copying difficult and both copies and subsequent reproductions differed in size from the original and were often distinctly elliptical. In view of this a simple measure of gap size (Walker & Veroff, 1956) seemed dubious, since a gap of standard actual size would have an angular size which would vary with its position on the ellipse. In other words, a response bias in drawing, by tending to elongate the ellipse in one particular direction, might easily give a spurious apparent change in gap size. In order to avoid this, gap size was expressed by the index $g/\{\frac{1}{2}(l+s)\}$, where g is the actual size of gap reproduced and l and s are the longest and shortest axes of the ellipse, all measured in centimetres. Essentially this expresses the gap as a ratio of the diameter; thus the index for a semicircle would be 1·0: for the large gap stimulus it was 0·48 and for the small 0·12.

Table 2. *Reproduction of circles with large and small gaps after a* 15 *sec delay*

	Large gap			Small gap		
Gap reproduced ...	Larger	Smaller	Equal	Larger	Smaller	Equal
Test order						
1st	11	9	0	10	10	0
2nd	6	14	0	11	8	1
Total	17	23	0	21	18	1

Table 3. *Recognition memory for circles with large and small gaps after a* 15 *sec delay*

	Large gap			Small gap		
Recognition judgement of gap ...	Larger	Smaller	Equal	Larger	Smaller	Equal
Test order						
1st	10	5	5	6	3	11
2nd	12	4	4	3	7	10
Total	22	9	9	9	10	21

Table 2 shows the direction of change between the original copy and subsequent recall. There is clearly no systematic trend for either of the gaps ($\chi^2 < 1$ in both cases). A more detailed analysis, taking into account the magnitude as well as direction of change, supported this conclusion. Systematic copying errors did, however, occur. Copied circles were significantly smaller than the original stimuli for both the small-gap stimulus (shown by 33 of the 40 subjects, $P < 0{\cdot}01$, sign test) and the large-gap stimulus (32 of the 40 subjects $P < 0{\cdot}01$, sign test). The actual size of the large gap was reduced ($P < 0{\cdot}01$, using Sandler's (1955) modification of the t-test), though the ratio of gap to diameter did not change. In the case of the small gap, there was no reliable change in either actual or relative size.

Recognition. Table 3 shows the frequency with which subjects judged an identical stimulus to be greater or smaller than the original, or equal to it. A judgement of 'larger' implies that the gap in the trace of the original has become smaller during the retention interval. This clearly occurs in the case of the large-gap stimulus, with 22 subjects showing this tendency and nine the reverse ($P < 0{\cdot}05$, two-tailed, sign test). No such effect occurs with the small gap, however, where there are also many more 'equal' judgements.

In showing a reliable tendency for the large gap to close, these results are consistent with those of Crumbaugh (1954) and James (1958). The smaller gap does not, however, show the expected tendency to open; the reason for this is not clear, though it is possible that an even smaller gap such as that used by James might have produced an effect. More disturbing than this, however, is the failure to find any effect using the method of reproduction. This suggests either that the apparent closure shown in condition II does not reflect a genuine change in the underlying trace, or else that the method of reproduction is not sensitive enough to detect it. Expt II attempts to increase the sensitivity of the method of reproduction by stressing the importance of reproducing the gap size accurately. In doing so it makes this condition more comparable with recognition, where subjects were told in advance that the size of the gap was the crucial factor.

EXPERIMENT II

Method

Procedure. This was identical with the reproduction condition of Expt I, with the exception that subjects were instructed to pay particular attention to the relative size of the gap. A further 40 servicemen served as subjects.

Results and discussion

Table 4 shows the frequency with which subjects reproduced the two gaps as larger or smaller than their original copies. Again there is obviously no suggestion of a trend. It seems, therefore, either that this method is so insensitive that it is completely unaffected by a small change in the underlying trace, or else that the effect obtained using the method of identical stimuli reflects something other than a change in the memory trace.

Table 4. *Reproduction of circles with large and small gaps after a* 15 *sec delay with instructions stressing the gap*

	Large gap			Small gap		
Gap reproduced ...	Larger	Smaller	Equal	Larger	Smaller	Equal
Test order						
1st	9	11	0	13	7	0
2nd	10	10	0	8	12	0
Total	19	21	0	21	19	0

One possible alternative hypothesis suggested by James (1958) is a response bias or guessing explanation. It could be argued that the subject is given an impossible discrimination task and is thus forced to guess. There is ample evidence that such guessing patterns are frequently non-random (Tune, 1964). James (1958) found evidence for such guessing habits in a situation closely analogous to the present study. His subjects were shown a circle with a gap; half were then shown the identical figure again and *judged* whether the second gap was greater or smaller. The remainder were initially shown only the first figure and told to *guess* whether the second gap was going to be greater or smaller. While subjects judged a small gap to be larger when seen a second time, and a large gap to be smaller, their guesses were in exactly the opposite direction. Thus, although James found a relationship, it did not fit a simple guessing interpretation of his results. It is arguable, however, that this guessing procedure is inappropriate, since it may easily be interpreted by the subject as an invitation to outguess the experimenter. In this sort of situation, the subject is likely to make the *less* obvious prediction, a state of affairs which would produce exactly the result that James obtained, a negative correlation between the subject's predictions before the event and his guessing behaviour when attempting to make a discrimination. Whether James's guessing control was strictly appropriate or not, however, he clearly showed the existence of guessing habits in this situation. The following experiment tests the hypothesis that the apparent closure shown by the large gap in Expt I was due to guessing by attempting to obtain data on guessing habits from a situation as close to the original as possible.

EXPERIMENT III

Method

Procedure. This was based on the method of identical stimuli used in Expt I. It differed in that subjects were told that the second stimulus would be presented so briefly that they might think they had seen nothing. They were nevertheless to make a judgement even though they felt it to be a complete guess. At this point a few words were said about perceptual defence and the evidence for perception without awareness. The first stimulus card was then presented as before. The second stimulus card was inserted into a box where it could only be seen when illuminated from behind. Instead of bearing an indentical stimulus, however, this second card was blank, but since it was only illuminated by a brief flash of light this was not detectable by the subject. Thus the procedure involved the presentation of the stimulus, a 15 sec delay, followed by the very brief illumination of a blank card at which point the subject was required to 'judge' whether the gap in the circle he had been told to expect on the second card was greater or smaller than or equal to the gap in the original stimulus. A further 40 servicemen were tested, half being shown the large gap first and half the small gap.

Table 5. *Guessing pattern for circles with large and small gaps*

(Figures in parentheses denote recognition responses from Expt I)

Distribution of guesses about gap ...	Large gap			Small gap		
	Larger	Smaller	Equal	Larger	Smaller	Equal
Test order						
1st	12 (10)	3 (5)	5 (5)	7 (6)	2 (3)	11 (11)
2nd	9 (12)	7 (4)	4 (4)	5 (3)	9 (7)	6 (10)
Total	21 (22)	10 (9)	9 (9)	12 (9)	11(10)	17 (21)

Results and discussion

Table 5 shows the distribution of 'judgements'; the figures in parentheses represent the recognition judgements observed in Expt I. There is a reliable tendency for the large gap to be followed by the 'judgement' larger ($\chi^2 = 3{\cdot}90$; 1 d.f.; $P < 0{\cdot}05$) though this is much less marked when the subject has already 'judged' the small gaps. There is no significant response bias for the small gap. Comparison between the distribution of responses in this and in the recognition condition of

Expt I indicates no reliable difference for either the large gap ($\chi^2 = 4{\cdot}15$; 5 d.f.; $P > 0{\cdot}5$) or the small ($\chi^2 = 2{\cdot}01$; 5 d.f.; $P > 0{\cdot}5$).

Since subjects apparently make the same 'recognition' response whether a second stimulus is present or not, it seems unnecessary to assume that the recognition results in Expt I reflect anything other than guessing habits. This, combined with the clear absence of any trend in the reproduction results, suggests that no systematic changes in the memory trace occurred in this study. Furthermore, in the absence of appropriate guessing controls, the results cited in Table 1 can no longer be regarded as convincing evidence for systematic change in the memory trace. This leaves only the Walker & Veroff study, which used the method of reproduction. Subjects in the present study found even copying the stimuli accurately rather difficult and tended to produce a very rapid crude drawing when asked to recall the figure, as if accepting that the task was beyond them and merely producing a general response so as to appear to comply with the instructions. In other words, the weaker the trace, the greater the probable influence of drawing habits. While this *may* not be responsible for Walker & Veroff's results, unless it can be discounted it seems unwise to assume a simple relation between a subject's drawing and the underlying memory trace.

The present results do not seem to require any explanation in terms of memory effects. They do, however, leave us with an unexplained pattern of guessing habits. If we assume that the studies in Table 1 also reflect guessing habits then we are left with the question of why subjects shown a large gap apparently expect the next gap to be smaller, and when shown a small gap expect the next to be larger. If, however, the subject assumes the sequence of gap sizes to be random and equiprobable (which is a reasonable initial hypothesis), then a large gap *will* tend to be followed by smaller one, simply because the majority of gaps *are* smaller, and vice versa. If this is so, this strategy should not be peculiar to gaps in circles, but should apply to any stimuli about which subjects can be assumed to have prior expectations. A subsequent study, yet to be reported, has indeed shown an exactly analogous effect for length of line using the method of identical stimuli, with long lines being judged shorter when viewed a second time, and short lines being judged longer.

In conclusion, it seems that the method of identical stimuli does indeed produce systematic effects, but that these reflect the guessing strategy adopted by the subject rather than changes in the memory trace.

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REDUCTION OF RESPONSE INTERFERENCE THROUGH VERBAL REPETITION

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The results of this study indicate that continuous verbal repetition of the interfering colour words on the Stroop test facilitates performance on the colour-naming task. There was no overall tendency for the groups repeating the colour words to show a decrement or increment in the meaning of the words as measured by the semantic differential scales. However, individual differences in meaning change were significantly related to time-improvement scores in colour-naming. The effect of repetition worked differently with two different forms of the Stroop card. With the regular Stroop card, an increase in rated meaning (a generation effect) was associated with the most improvement, while with a revised card, a decrease in rated meaning (a satiation effect) was associated with the most improvement in time scores. The implications of these results for theories of interference and verbal satiation are discussed.

Numerous experiments on the effects of continuous repetition or visual fixation of words report that subjects experience a decrease in the availability or meaning of the words involved (e.g. Severance & Washburn, 1907; Bassett & Warne, 1919; Don & Weld, 1924; Lambert & Jakobovits, 1960). This change has been measured by means of rating scales (Lambert & Jakobovits, 1960); a change in the associations given to repeated words (Smith & Raygor, 1956); or simply a subjective report (Severance & Washburn, 1907; Bassett & Warne, 1919; Wertheimer & Gillis, 1958; Fillenbaum, 1963). Several experiments, however, have investigated the subsequent effects of repetition on performance of various tasks that call on skill in manipulating a meaningful word or symbol (Jakobovits, 1962; Jakobovits & Lambert, 1962*a, b*; Kanungo *et al.*, 1962; Kanungo & Lambert, 1963, 1964).

Most of the tasks used to study the effects of repetition on performance have been devised so that a decrement in the availability or meaning of the word is associated with a decrement in performance. If, however, the repeated words were not part of the correct response sequence but belonged instead to an inappropriate, competing sequence, a decrease in the availability or meaning of the word, experimentally induced, would be expected to facilitate performance. Since the psychological consequences of an inability to ignore or to set aside are as significant as those stemming from an inability to remember, the question involved here is of theoretical and practical importance.

One task that seems particularly well suited to the study of this question is the Stroop test (Stroop, 1935), which illustrates very effectively the interference caused by a simultaneously elicited, stronger and antagonistic response. As modified by Thurstone (1944), this test consists of a series of colour words (such as *red, blue, orange*, etc.), arranged in rows on a large card. Each word, however, is printed not in black but in one of the test colours, with the restriction that no colour word is ever printed in its own colour. For example, if the four colour words are *red, blue, orange* and *brown*, then *red* may be printed in BLUE, ORANGE or BROWN ink but never in RED.

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Subjects experience great difficulty when instructed to name the colours of the inks on the card, for in order to do so they must ignore the printed word. The time required for this task is usually much greater than that taken to name the colours of colour patches alone, i.e. when there are no interfering words (e.g. Stroop, 1935; Rand *et al.*, 1963). Thus the word-reading process interferes with the colour-naming process; the competition need not be conscious, for subjects find it difficult to recall the interfering words and may even be surprised when asked to do so (Grand & Segal, 1964).

The present study is an extension of two unpublished McGill experiments (Hurr, 1962; Fraser, 1963) that investigated the effects on the Stroop test of continuous repetition of the interfering words prior to naming the colours. All three studies employed the regular card described above, and a revised card on which the interfering words referred to colours other than those of the inks actually used (e.g. the word *red* was written in the colour YELLOW, but the word *yellow* never appeared on the card). The differences in the cards are of special interest. (For a comparison of the methods used in the three studies and a discussion of the findings from all three, see Thompson (now Ellison) (1964).) With the regular card it could not be assumed that repetition of the interfering words would affect these alone, for the interfering words are the same as the names of the coloured inks. The effects of the verbal repetition, an encoding process, might therefore generalize, at the verbal-motor or some other level of representation, to the colour-naming task, a decoding process. The possibility of generalization would be less on the revised card. If the verbal repetition treatment had no differential effects on the two cards, this would be evidence against the generalization hypothesis, and make the effect very specific to the repeated printed word. Also, extrapolating from Klein's (1961) results, it would be expected that the revised card would be easier than the regular Stroop card.

The purpose of the present experiment was to attempt to replicate the findings of Hurr & Fraser, showing that a decrement in the meaning or availability of the interfering words results in improved performance on the Stroop test. This outcome would be expected only in the case of the revised card; in the case of the regular card it would only be expected if generalization does not occur. Attention was also given to the manner in which changes of meaning affect performance on both types of Stroop cards.

METHOD

Materials. Two Stroop-type cards were prepared. On the first, the regular Stroop card, four colour words (*red, orange, blue* and *brown*) were arranged randomly in 10 rows of 10 words each, with the restriction that no word was ever followed by that same word. The colours in which the words were printed were the same (RED, ORANGE, BLUE and BROWN), although no word ever appeared in the colour which it denotes.

On the revised card the same four colour words (*red, orange, blue* and *brown*) were used, but the colours in which the words were written were PINK, YELLOW, GREY and GREEN.

The two sets of words were equated as well as possible for frequency of occurrence (Thorndike & Lorge, 1944), length, and number of syllables. Each colour word appeared 25 times on a card, and on the regular card was printed an approximately equal number of times in each of the three colours other than the one it denotes. For the revised card RED was matched with PINK, ORANGE with YELLOW, GREY with BLUE, and BROWN with GREEN. Then the same order of words and colours was used as with the regular card, with PINK being substituted for the ink colour RED and so on. In this way the number of different word–ink combinations was kept the same for both cards, and combinations that might have caused special difficulty on the revised card because of their similarity (e.g. *red* printed in PINK, *orange* printed in YELLOW) were avoided.

Thus subjects working with the regular card would encounter a sequence such as: *red* printed in ORANGE ink, followed by *blue* in BROWN ink, *orange* RED, *brown* BLUE, etc., and be expected to name the ink colours, ORANGE, BROWN, RED and BLUE, as rapidly as possible. In contrast, with the revised card, the subjects would encounter a sequence such as: *red* YELLOW, *blue* GREEN, *orange* PINK, *brown* GREY, and be asked to respond with YELLOW, GREEN, PINK, GREY, etc.

Subjects. The subjects were 90 female undergraduates at McGill University who volunteered their time. Each subject was assigned at random to one of the six conditions described below.

Procedure. The instructions were presented in written form to ensure uniformity, and each subject was tested individually. Six groups of 15 subjects were formed and two groups were assigned to each of the three conditions discussed below, one set of groups working with the regular Stroop card and the other with the revised card.

(1) *Experimental condition.* First, the time required for the subject to read the regular or revised practice card was recorded. Then she was instructed to name the colours from the appropriate interference card, and the times for the first-half and the full card were noted, yielding two initial time scores for the card. Half-time measures were taken because it was anticipated that the effects of verbal repetition might dissipate before the subject had completed the 100-item card.

Then the subject rated the colour words *red*, *blue*, *orange* and *brown* on six semantic differential scales (hard–soft, weak–strong, unpleasant–pleasant, beautiful–ugly, passive–active and excitable–calm), two scales representing each of the major factors described by Osgood *et al.* (1957). The experimenter presented the words and scales by means of a 'Kardex' folder, following the procedure developed by Lambert & Jakobovits (1960). After the initial ratings the subject repeated each word at the rate of two to three repetitions per second for a period of 15 sec and, following this, re-rated the word on one of the scales. This procedure was continued until each word had been re-rated on each of the six scales. Thus the total repetition time for each word was 6 × 15, or 90 sec. The words were presented in a random order, which was reversed for half of the subjects in each group. The experimenter then presented the interference card for the second time and recorded the final one-half and total time scores.

(2) *Different-word control.* The procedure here was the same as for the experimental groups, but the words that were rated, repeated and re-rated were *dog*, *moon*, *charm* and *doctor*. These words were equated to the colour words with respect to frequency of occurrence, length, and number of syllables.

(3) *Silence control.* The colour-naming procedure was the same for this condition as for the experimental and different-word conditions. This group, however, received no verbal repetition treatment. Each subject first completed the initial ratings of the colour words on all the scales, and then waited 15 sec in silence before a word and a scale were exposed for a second rating. In other words, silence was substituted for the repetition used in the other two conditions. The word and the scale to be rated were not shown to the subject until the end of the 15 sec period to avoid the possibility that she might subvocally repeat or think about the word to be rated.

Thus all subjects progressed through the following series of tasks: (1) initial Stroop test (regular or revised); (2) initial semantic ratings; (3) repetition treatment (relevant, irrelevant or silence control) *and* second semantic ratings; and (4) second Stroop test. With this design one can determine whether the continuous repetition of the interfering colour words, in contrast to the control conditions, affects the speed of colour-naming on each type of Stroop card.

RESULTS

The average time scores for naming the colours of the complete, 100-item interference cards and the scores for the first half of the cards (five rows of 10 words each) are given in Table 1. The initial and final time scores were compared by means of a 3 × 2 × 2 analysis of variance (Winer, 1962, p. 343). There was a significant interaction effect of 'repetition' conditions on amount of improvement from initial to final reading of the first halves of the interference cards ($F = 3{\cdot}54$; d.f. 2, 84; $P < 0{\cdot}05$). This effect, however, did not approach significance for changes in the time taken to read the complete cards ($F = 1{\cdot}60$, d.f. 2, 84).

When the three repetition groups are compared for their relative improvement scores on the first half of the card, it is seen that the experimental groups improved significantly more than did the different-word group ($t = 2{\cdot}59$; $P < 0{\cdot}02$), while the improvement of the experimental group over that of the silence control group approached significance ($t = 1{\cdot}75$; $P < 0{\cdot}10 > 0{\cdot}05$, two-tailed test). (Although this difference was clearly predicted from a theoretical point of view and was a likely outcome in view of its occurrence in the two previous studies referred to, two-tailed significance tests were used because of the essentially exploratory nature of this investigation.) Since there was no significant difference between the two control groups ($t = 0{\cdot}70$; P, n.s.), they were combined and again the experimental groups showed significantly more improvement than the two control groups ($t = 2{\cdot}48$; $P < 0{\cdot}02$).

Table 1. *Average time (sec.) for colour-naming on interference cards*

	Complete-card times			Half-card times		
Condition...	Initial	Final	Difference	Initial	Final	Difference
Regular Stroop card						
Experimental ($n = 15$)	97·80	83·86	−13·94	48·27	39·67	−8·60
Different-word control ($n = 15$)	95·46	83·60	−11·86	46·00	40·33	−5·67
Silence control ($n = 15$)	104·80	90·60	−14·20	50·13	42·27	−7·86
Revised Stroop card						
Experimental ($n = 15$)	92·46	75·60	−16·86	45·67	35·13	−10·54
Different-word control ($n = 15$)	92·93	77·80	−15·13	44·87	37·20	−7·67
Silence control ($n = 15$)	100·60	81·00	−19·60	47·73	40·33	−7·40

Table 2. *Average polarity score on semantic differential across four words and six scales*

	Regular Stroop card			Revised Stroop card		
	Experimental	Different-word control	Silence control	Experimental	Different-word control	Silence control
Initial	40·53	39·67	43·00	44·20	39·20	42·40
Final	39·06	36·47	44·73	40·87	42·60	41·47
Difference	−1·47	−3·20	1·73	−3·33	3·40	−0·93
t	0·68	1·67	0·99	1·62	2·18	0·91
d.f.	14	14	14	14	14	14
P	n.s.	n.s.	n.s.	n.s.	$< 0{\cdot}05 > 0{\cdot}01$	n.s.

For the complete-card time scores, there was a significant interaction effect of types of card on improvement from initial to final reading ($F = 6{\cdot}19$; d.f. 1, 84; $P < 0{\cdot}05$), meaning that the subjects reading the revised card, whether they were in the experimental or one of the control conditions, improved more than those reading the regular card. This effect was not significant for the half-card scores ($F = 1{\cdot}63$; d.f. 1, 84).

However, there was no significant triple interaction (conditions × card type × initial *v.* final reading) for either the complete-card or the half-card time scores. There is, then, no evidence that the *experimental* regular card group reacted differently to the verbal repetition treatment than did the *experimental* revised card group in terms of average improvement scores.

The change in the intensity of meaning of the words from the first to the second ratings on the semantic differential was measured by a polarity difference score (Lambert & Jakobovits, 1960). This score is an index of the change in the extremity of the ratings relative to the midpoint on the scale, regardless of the direction of the change. These results are shown in Table 2. The only significant change is that of the different-word control, on the revised card, which showed a significant *increase* in rated meaning after repetition, indicating a generation of intensity of connotative meaning. The different-word control for the regular card, on the other hand, showed a slight trend toward satiation, i.e. a *decrease* in rated meaning after repetition. The experimental group reading the revised card showed only a trend toward satiation and the other groups showed no change from initial to final ratings.

In general, then, there is no consistent pattern in the effects of repetition on the average semantic ratings of the experimental and control groups. However, the findings of previous research on semantic changes (discussed below) directed our attention to the possibility that *within* each group the effect of verbal repetition on meaning might be exercising some differential effect on individual performance on the Stroop test. With this end in mind, correlations between polarity difference scores and time-improvement scores were calculated for all groups, for both the complete- and the half-card scores. A positive correlation would mean that a generation effect on the semantic differential is associated with the highest improvement scores on the interference cards and a negative correlation would mean that a satiation effect is associated with the highest improvement scores. A significant correlation for the experimental groups, but not for the control groups, would suggest that some associated change of meaning was taking place in spite of the lack of an overall group decrease or increase in rated meaning. These results are shown in Table 3.

Table 3. *Correlations between meaning change scores on the semantic differential and time-improvement scores on the Stroop interference test*†

	Regular Stroop card		Revised Stroop card	
Condition...	Complete card	Half card	Complete card	Half card
Experimental	0·73**	0·64*	−0·28	−0·55*
Different-word control	−0·17	−0·11	−0·50	−0·24
Silence control	−0·02	0·42	−0·11	0 03

* $P < 0{\cdot}05$; ** $P < 0{\cdot}01$.

† A positive correlation indicates that a generation effect on the semantic differential is associated with the highest improvement scores on the interference card. A negative correlation indicates that a satiation effect is associated with the highest improvement scores.

For the experimental regular card group the correlations for both the complete- and the half-card time scores are significant ($P < 0{\cdot}01$; $P < 0{\cdot}05$). These correlations are positive, indicating that a generation of meaning after repetition is associated with the most improvement in performance on the interference card. Those subjects who improved least, on the other hand, are those showing the expected satiation effect, or decreases in rated meaning after repetition. For the experimental revised card group the correlation for the half-card time scores is significant ($P < 0{\cdot}05$) and here satiation is associated with the greatest improvement on the interference card, and generation with the least.

DISCUSSION

In agreement with the two previous studies, this experiment shows the following. (1) Continuous repetition of the interfering words on the Stroop test is accompanied by an improvement in performance in terms of time taken to name the coloured inks from the interference card. The effect dissipates in time, being clearly evident after 50 items of the interference cards but not after 100 items. (2) Subjects repeating the interfering words showed significantly more improvement than did the control subjects. (3) Subjects reading the revised card improved more from first to second testing, whether they belonged to the experimental or control groups, than did subjects reading the regular card. It was noted that a longer time was required to read the practice series preceding the revised card, suggesting that this set of words is less familiar than the set of words on the regular practice card, even though the two sets were equated for length, number of syllables and frequency of occurrence. Since, in this study, the interfering words are the same for both cards, the difference is due to the coloured inks which must be named. A differential practice effect would thus result in more improvement on the revised card reflecting an improvement in colour naming.

However, there was no significant difference in the amount of improvement between those *experimental* subjects reading the regular card and those reading the revised card, and it cannot be assumed from the average improvement scores that the verbal repetition is working differently in the two cases. For example, a specific motor facilitation effect (subjects simply learning through repetition to encode the repeated colour words more readily) would lead us to expect the regular card subjects to improve more, and this was not the case.

The results imply that it is the availability or meaning of the repeated words that is affected by the continuous repetition. The simplest hypothesis would be that after continued elicitation, the interfering words are decreased in their meaning or availability to the subject; i.e. that the words show 'semantic satiation' (see Lambert & Jakobovits, 1960). In support of this interpretation there is some independent evidence that the less meaningful or the less familiar the words on the test, the less interference they will cause (e.g. Rouse & Maas, 1961).

This explanation, however, seems insufficient. As in the two earlier McGill studies using essentially the same design, the average polarity difference scores for colours show no significant satiation effects. Apparently something about the design (in which the semantic ratings and the continuous repetition follow the first administration of the Stroop test) evokes wide individual differences in reaction to the repetition treatment, some subjects showing satiation effects and others showing generation effects. We were alerted to the possibility of individual differences of this sort, and of their importance, by the results of previous studies. For instance, Jakobovits & Lambert (1962*b*) found that repetition causes at first an increase in the repeated word's availability and then, as repetition continues, a decrease. Thus individuals could be at quite different points on the typical gradient at the end of a constant period of repetition. Jakobovits & Lambert (1962*a*) also found that individual differences in reaction to repetition are significantly related to one's efficiency in the subsequent use of the repeated words.

When the individual differences were examined, significant correlations were found between the meaning-change scores and the time-improvement scores for the experimental groups. This result suggests that individual differences in reaction to verbal repetition are revealed in both the semantic differential and the performance data. Furthermore, there is a marked difference in the way that continuous verbal repetition affects the time taken to name the colours from the two cards, although this difference is not revealed in the average improvement scores for the groups. For the revised card group the subjects who showed a decrease in rated meaning (a satiation effect) were those who showed the most improvement. This result supports the prediction that a decrease in the meaning or availability of the interfering colour words would result in decreased time scores for the colour-naming task. The result is also similar to one discussed by Rouse & Maas (1961), who found that increasing practice and familiarity with the nonsense syllables which served as the interfering words in their test produced a monotonic increase in the time required to name the colours. In the terms of the present study, generation of the rated meaning of the words was accompanied by an increase in their efficiency as competitors, whereas satiation was accompanied by a decrease in their efficiency as competitors, and greater improvement in performance.

In contrast, on the regular Stroop card, those who showed an increase in rated meaning (a generation effect) were those who improved the most. It will be recalled that both colour words and their actual referents are found on the regular card. Therefore the effects of continuous verbal repetition could generalize from the colour words to their referents; on the other hand, the repetition might simply affect the efficiency with which subjects encode those verbal labels (e.g. *red, blue*) shared by both the interfering words and the coloured inks. The present experiment did not allow us to distinguish between these two possibilities, both of which would yield an identical result. The results, however, do suggest the following explanation: when repetition produces semantic generation, the colour words become increasingly differentiated from each other, since the meaning or availability of the words increases for the subject. If this distinctiveness generalizes to the colour referents, then each element in a pair of competing responses (e.g. the word *red* printed in BLUE) should become more differentiated and interference between them should be decreased. Differentiation of this sort would facilitate the setting aside of one habit (word-reading) that might interfere with the other (colour-naming). Similar effects have been reported in many other studies where well-differentiated habits produce little interference with each other.

A developmental study using the Stroop test (Rand *et al.* 1963) lends support to this explanation. These investigators found that the speed of both colour-naming and word-reading increased with age, while the typical interference effect found with the regular Stroop card first increased as children began to acquire the word-reading habit, and then decreased as both colour-naming and word-reading reached a high level of efficiency. At the same time, the tendency to read the word instead of naming the colour diminished. Langer & Rosenberg (1964) explain this developmental sequence as follows: 'early in the developmental process, the representational relationships between the symbols and the referents are relatively unstable. One aspect of this instability is the lack of differentiation between various referents of the same sphere of meaning such as color. Consequently it is most difficult to maintain attention directed towards the perceptual color referents, which is the task requirement, when presented with incongruent color referents' (p. 11). They also suggest that for 'children whose reading is improving...the relationships between symbols and their color referents become relatively well established such that the various colors are well differentiated and the possibility for interference of one upon the other diminishes' (p. 11). Further research is of course needed to test the adequacy of this explanation and to investigate its implications for theories of interference and of verbal satiation.

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UNHAPPY MEMORIES OF 'THE HAPPY PAST': STUDIES IN COGNITIVE DISSONANCE

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The aim of the two studies reported was to test two extensions of the dissonance model. The hypothesis was that over longer time periods the congruence between initially dissonant emotional attitudes with regard to a past (not recent) life period will become more pronounced. The main variables were the evaluative attitude to the whole past period in terms of happiness, expressed as a ratio of unpleasant (U) to pleasant (P) checked adjectives, the ratio of U:P memories recalled from that period, U and P being judgements from the viewpoint of the present, and the wish to return to that period.

The sample of Study I included 126 Kibbutz children, who were all the children in three Kibbutzim similar in child-rearing methods in the following three age-groups: 8.1–10, 10.1–12 and 12.1–14. Each child was interviewed individually according to a questionnaire about his childhood (the period 1–6 yr). The results revealed no significant differences between the groups in the U:P memories ratio (average = 2·15) or U:P adjectives ratio (average = 0·38), and no significant correlations between the variables of wish to return to childhood, U:P adjectives and U:P memories, but for the correlation between the latter two in the 8.1–10 yr old group. The results refute the hypothesis in revealing the persistence of dissonances.

In Study II 35 adults in the age range 30–50 yr, chosen randomly from three localities, were interviewed in an identical manner about the period of 20–25 yr as well as about their evaluation of happiness in personal childhood, wish to return to childhood, evaluation of happiness in childhood in general and of humanity in the past. In addition to corroborating fully all the results of Study I in regard to the age of 20–25 yr it was found that evaluation of happiness in personal and general childhood are correlated, as well as the lack of wish to return to one period with that to another. Possible explanations and implications of the persistence of the dissonances are discussed.

Common to the theoretical models of balance (Heider, 1946), congruity (Osgood & Tannenbaum, 1955) and dissonance (Festinger, 1957) is the assertion that attitudes, beliefs, opinions and informations tend to be organized into logically consistent sets. This state of affairs, or an approximation to it, is assumed to be produced as a result of pressures generated by the occurrence of inconsistencies, which in their turn depend upon the contiguous appearance of the potentially conflicting cognitive items. Predictions from this model about various changes in opinions and attitudes, and about selective distortions, have been tested and confirmed with regard to inconsistencies between various evaluative attitudes (Osgood & Tannenbaum, 1955) or beliefs (McGuire, 1960), between attitudes and informations (Brehm & Cohen, 1962; Festinger, 1957), as well as between evaluative attitude and emotional attitude (Rosenberg, 1960), but so far not with regard to inconsistencies between various emotional attitudes. The first aim of the studies reported here was to test the dissonance model with regard to this missing link.

The second aim of these studies relates to the testing of a different extension of the consonance model. Characteristic of the various studies performed in this domain is the limitation of the time period allowed for the operation of the balancing or consonance-generating forces. In most of the recorded studies this time period is at most no longer than 30–60 min and in none does it extend to weeks or months. Concomitant upon this time limitation is the other factor of restricting the repetition of dissonance to one trial. Yet these limitations seem to be rather a product of experimental conditions than of the theory, which in no way implies restriction of the effect to short-term periods and to one-trial cases. On the contrary, Osgood *et al.* (1957, pp. 207–209, 216) explicitly assume that with repetitions over longer stretches of time the congruity effect would grow in potency and stability. Greater ambiguity is characteristic of the view adopted by Brehm & Cohen (1962, pp. 97–104): they hypothesize two possible and opposite effects of repeated dissonances, i.e. an increased reaction to the dissonance, possibly due to accumulation of tension or increased sensitivity to the involved dissonances, and a decreased reaction, possibly due to dissonance avoidance tendencies, learning to tolerate dissonance, etc. Yet longer time periods with wider spacings of the dissonance situations are assumed to favour the former reaction. Accordingly, the second aim of our studies was to test the hypothesis that over longer time periods, when wide intervals may be assumed to occur between the various confrontations with the dissonance(s), the congruence between initially dissonant cognitions will grow more pronounced.

To test this hypothesis a contents domain was chosen where emotional attitudes could be assumed to be easy to tap, i.e. the relation to one's past. In order to allow for a more pronounced emotional background for the various evaluations, past (but not recent) periods of life were taken as the object of the studies. The passage of time was considered as the independent variable and the various emotional attitudes as the dependent variables. The studies consisted in measuring the congruence between the evaluative attitude to a past (not recent) period of life in terms of happiness and the amounts of pleasant (P) and unpleasant (U) memories recalled from that period, P and U being taken as judgemental attitudes from the standpoint of the present and not of the past period.

The rationale for the choice of content domain was based on two further assumptions. First, that dissonance might be initially characteristic of the two sets of variables, i.e. evaluative attitude and judgements on memories, insofar as their determinants are different, overall attitudes being partly dependent on stereotypes (e.g. the 'happy past') with memories being more dependent on personal experiences (i.e. various findings testify to no preponderance of P over U past memories (e.g. Gordon, 1928; Menzies, 1933; O'Kelly & Steckle, 1940) or only to a slight preponderance (e.g. Jersild, 1931; Peters, 1914)). Secondly, the often-repeated assumption that attempts at dissonance reduction are more successful in cases where the relevant reality is internal and somewhat ambiguous (Brehm & Cohen, 1962, p. 4), both of which conditions are characteristic of attitudes and of judgements on memories.

STUDY I

In view of the above considerations, the hypothesis was that in rather homogeneous samples of children of different ages, congruence between the two sets of emotional attitudes—overall attitude to childhood, as indexed by evaluation in terms of happiness and wish to return to childhood—and the relative amounts of P and U memories of childhood, will show increase with age.

Method

Subjects. The subjects were 126 children from three age-groups: 8.1–10 (n = 40: 18 boys, 22 girls); 10.1–12 (n = 44: 20 boys, 24 girls); and 12.1–14 (n = 42: 22 boys, 20 girls). All the subjects were children born and living in various Kibbutzim in Israel. Kibbutz children were chosen, first, because the controlled and standardized child-rearing methods characteristic of the Kibbutz make for a relatively high homogeneity of past and present life conditions of these children, thus enabling comparison across ages; and secondly, because of the particularly favourable atmosphere in which all these children have grown up.

In order to eliminate the dangers of the Independence War in 1948 as a possible source of U memories and still to ensure a time interval of at least two years between experiences and recall, the upper age limit had to be 14 yr (children born in 1949) and the lowest age limit 8 yr (children born in 1956). The material was collected during 1963.

The subjects were all the children in these age-groups in three Kibbutzim randomly chosen from a greater number of Kibbutzim in which child-rearing methods were found to be similar in the following respects: staying not with the parents, but first in the 'Babies' House' and then in the 'Children's House'; amount and kind of child-care done by the parents and of time spent with them; the degree of stability in nursing personnel and the ages at which nurses habitually change; the age and methods of weaning and toilet training; and the tasks given the children at different ages. The findings refer to an unpublished survey of child-rearing methods in the Israeli Kibbutzim carried out by the authors during 1961–2.

Procedure. Each child was interviewed individually, in the presence of no observers, about his childhood memories. Chilhood was defined as the age period 1–6 yr, 6 yr being the age at which children acquire membership in the school's community and start living without nurses. Interviewing was performed by three kindergarten teachers, themselves Kibbutz members. All three were fully ignorant of the research hypothesis. For control purposes, each interviewer interviewed children from all three Kibbutzim and from all three age-groups.

The interview was conducted by means of a questionnaire which consisted of 19 questions, mostly open-ended, about various aspects of childhood and kindergarten life. The questionnaire included three types of questions: those designed to encourage recall and free talking; those designed to test the accuracy of recall of various facts concerning childhood and kindergarten life (e.g. stories told, or children's parties); and those relevant to this study, interspersed in various parts of the questionnaire. These relevant questions were: (1) What do you remember from childhood which seems to you now pleasant? (2) What do you remember from childhood which seems to you now unpleasant? (3) How do you feel now about the whole period of childhood? (4) Would you like to be a child again?

Questions 3 and 4 were designed to supply information about the overall attitude of the child to his childhood period, with question 3 tapping more the evaluation in terms of happiness. In answer to question 3 the child had to check as many adjectives as he wanted from a mixed list of 12 adjectives, six positive ones and six negative ones. The positive and negative adjectives were selected on the basis of a pre-test, performed a month earlier, in which a list of 40 adjectives, each of which had to be checked as positive or negative in terms of happiness, was presented to a sample of 40 children in the age range 8–14 drawn from the same Kibbutzim population which later formed the experimental sample. Only those adjectives which were unanimously checked as positive or negative were included in the questionnaire.

Analysis of data. The only criterion for determining what was a P or a U memory was the decision of the child, which in all cases was immediate and clear-cut. In most cases (in 78 per cent of the memories) the P and U memories were mentioned as answers to the adequate questions (*Procedure*, questions 1 and 2). The remaining 22 per cent of the memories were mentioned by the children in answer to other questions, mostly (81 per cent of these cases) in the form of a U memory in answer to the question requiring P memories or vice versa, and in a few cases in the form of sudden recall while answering a question not considered as relevant. In all of these cases the subjects always gave the P or U nature of the memory.

A unit of memory was defined as reference to a definite event, person, object or situation whose meaning is independent of that of memories related before or afterwards. Thus if a child mentioned various unpleasant things which happened to him during a certain illness, all these stories were counted as one memory. If, however, he mentioned a series of various people and situations which used to frighten him, each for different reasons, each reference was counted as a separate memory. The concordance between the counts of memories by two independent judges was complete in 96 per cent of the cases, the remaining cases being resolved upon discussion.

In order to overcome the comparison difficulties created by the fact that the overall number of P and U memories, as well as the overall number of P and U checked adjectives (question 3), differed greatly from child to child, the raw numbers of memories and adjectives were transformed for each child into a ratio of U:P or of P:U.

Results

Tables 1 and 2 present the results concerning percentages and ratios of U and P memories in terms of whole age-groups since no significant differences were found in these respects either between boys and girls in each age-group separately and in all together, or between the three different interviewers.

Table 1. *Percentages of P and U memories of childhood in the three age-groups of children*

Age-groups	Percentage of U memories	Percentage of P memories	Significance of difference	*P*
8.1–10	64·57	35·43	CR = 2·60	< 0·01
10.1–12	56·37	43·63	CR = 1·69	> 0·05
12.1–14	62·94	37·06	CR = 3·35	< 0·01

Table 2. *Ratio between U and P memories of childhood in the three age-groups of children*

	Mean ratio							
	U > P		U < P		U = P		Total U:P	
Age-groups	*n*	U:P	*n*	U:P	*n*	U:P	*n*	U:P
8.1–10	31	3·03	6	2·36	3	1	40	2·50
10.1–12	27	2·93	11	2·82	6	1	44	1·74
12.1–14	30	2·82	9	2·01	3	1	42	2·21
Significance		*F* = 0·789 n.s.		*F* = 0·345 n.s.		*F* = 0·238 n.s.		

It is remarkable that in all three age-groups U exceed P memories, regardless of whether percentages of total number of P and U memories or averages of total individual U:P ratios are considered. Also the number of subjects in each group whose memories show the relation U > P is by far the largest, the range lying between 61·36 per cent of the subjects for the middle group and 77·50 per cent for the youngest group. Moreover, the differences between the U:P ratios of all age-groups are non-significant, i.e. the U:P ratio is constant across age-groups in the range of 8.1–14 yr, its average value being 2·15 in favour of U memories.

In view of these surprising findings in children known to have grown up in particularly favourable conditions, two points were checked after the completion of the study: first, whether the groups included any disturbed children, and secondly, the amount of traumatic events recalled by the children. The checking of the first point was made easy by the particular attention paid by Kibbutz nurses and educators to any, even minor, mental symptoms manifested by children. Only five of the 126 children were found to have had psychological consultation during childhood because of phobic or enuretic symptoms. Regarding the second point it was found that only

2 per cent of the recalled U memories could be considered as referring to exceptional events with possible traumatic effects, such as the death of a parent, a fearful night during which the Kibbutz sustained an Arab attack, or a fire which destroyed many homes. In contrast, 98 per cent of the U memories referred to continuous occurrences, such as eating certain foods, being in the company of unpleasant children, having to listen to boring kindergarten stories, etc., which may be characterized as annoying rather than injurious.

The picture, however, changes completely when we pass to the overall evaluation of childhood in terms of happiness. Indeed the adjective ratios of U:P are again constant across the three age-groups (Table 3), but they are decidedly in favour of P adjectives as against U ones, equalling on the average 0·38, the medians for the three groups being 0·24, 0·20, 0·53 respectively. Moreover, for the majority of subjects in each group the distribution of adjectives manifests the relation of U:P, the percentages ranging from 90 per cent in the youngest group to 97·57 per cent in the oldest one.

Table 3. *Ratio between U and P adjectives describing personal childhood in the three age-groups of children*

	Mean ratio							
	U > P		U < P		U = P		Total U:P	
Age-group	*n*	U:P	*n*	U:P	*n*	U:P	*n*	U:P
8.1–10	2	1·49	36	3·84	2	1	40	0·44
10.1–12	1	2·00	40	4·07	3	1	44	0·40
12.1–14	—	—	41	4·10	1	1	42	0·30

Significance $z = 0{\cdot}20$ n.s. $F = 1{\cdot}522$ n.s. $F = 1{\cdot}203$ n.s.

Table 4. *Distribution of answers (in percentages) in regard to wish to return to childhood in the three age-groups of children*

Age-group	Yes	No	Yes with reservations
8.1–10	7·50	82·50	10
10.1–12	9·09	68·19	22·72
12.1–14	14 28	52·39	33·33

$\chi^2 = 8{\cdot}181;\ 0{\cdot}001 < P < 0{\cdot}01.$

In spite of this positive emotional evaluation of childhood, the majority of subjects in each group declare they do not want to return to childhood (Table 4). Table 4 also shows that the number of positive answers accompanied by reservations increases with age, as would be expected in view of an increased differentiation in perception and thought. More important is the fact that the percentages denoting the number of children who want to return to childhood increase significantly with age (Table 4), while the emotional evaluation of childhood in terms of happiness (Table 3) remains constant with increase in age. The assumption that both the overall evaluation and the wish to return to childhood could serve as equal indices for the attitude towards childhood was disproved by the non-significant correlations between these two variables in each age-group and for the whole sample (the contingency coefficients ranged from 0·01 to 0·21).

As could be expected in view of the above results in the two sets of variables separately, the hypothesis concerning a growing congruence between the overall attitude to childhood and the relative amounts of P and U childhood memories was disproved. Of the six correlations between U:P memories ratios on the one hand, and U:P adjectives ratios (Pearson's product moment) as well as the wish to return to childhood (contingency coefficients) on the other hand, only one was significant. This was the correlation between U:P memories ratio and U:P adjectives ratios for the 8.1–10 group ($r = 0{\cdot}35$; $0{\cdot}02 < P > 0{\cdot}05$). The results point then to a development with age in a direction contrary to that hypothesized, i.e. the congruence initially present disappears with age. In other words, the results point to the existence of a state of dissonance from the age of 10–14 yr between evaluative attitude to childhood, the wish to return to childhood, and the relative amounts of P and U childhood memories.

STUDY II

The second study had three aims:

(1) To test whether the particular dissonances found in Study I (*a*) between the relative amounts of P and U memories from a past period and the overall evaluation of happiness in that period, and (*b*) between the overall evaluation of happiness in a past period and the wish to return to that period, are specific for childhood or hold also in the case of a period other than childhood, i.e. for adults over 30 in regard to the age of 20–25 yr.

(2) To test whether the particular lack of congruity between the overall evaluation of happiness in childhood and the wish to return to chilhood is specific for children in range 8–14 yr, or characterizes also the approach of adults over 30 towards childhood.

(3) To test two hypotheses which, if proved, could serve as partial explanation for the dissonances found with children. Both hypotheses stem from the assumption that at least part of 'the resistance to change of a cognitive element comes from the extent to which such change would produce new dissonance' (Brehm & Cohen, 1962, p. 4), i.e. would conflict with other, more general, cognitions with which it is related. The hypotheses were: (*a*) There exists a generalized attitude in regard to returning back in time to past periods, i.e. the wish to return to childhood will correlate with the wish to return to the age of 20–25 yr. (*b*) There exists a generalized attitude of evaluation of happiness in the past, i.e. the overall evaluation of happiness in personal childhood, in childhood in general, in the age of 20–25 yr and of humanity in past periods as compared with the present, will all prove to be correlated the one with the other.

Method

Subjects. The group of subjects consisted of 35 adults (19 men, 16 women) in the age range 30–50 yr. They were chosen from adults in the specified age range in three very different social settings: the humanities faculty of the Tel Aviv University, the regional police headquarters in Tel Aviv, and a synagogue community in a smaller town. Originally the group comprised 39 subjects (13 subjects from each setting) but was reduced to 35 because four subjects were found to have been in Nazi concentration camps when 20–25 yr old, and therefore had only U memories from that period. The remaining 35 subjects, none of whom had ever undergone psychotherapy, lived at the age of 20–25 yr in objectively normal conditions, i.e. conditions essentially comparable to those of the children's groups in childhood.

Procedure. The subjects were interviewed separately in the presence of no observer. There were two different interviewers, both ignorant of the hypothesis. The interview was conducted according to a questionnaire which included 12 questions. Some of the questions were designed to encourage recall and free talking about the age of 20–25 yr; four were designed to afford answers to both aspects of aim (1) and were therefore a replication of the questions children were asked about childhood, the only difference being that they related to the period of 20–25 yr of age; six additional questions were designed to supply information concerning aims (2) and (3), i.e. the subjects were asked whether they wanted to return to childhood and were requested to point out on a five-rank scale ranging from 'most happy' to 'most unhappy' their evaluation of happiness in their personal childhood, in childhood of most people, and to indicate whether they considered humanity was in past ages happier than now, less happy or equally happy.

Analysis of data. The determination of the unit of memory and the number of memories, as well as the transformation of the number of U and P memories and of U and P checked adjectives relating to the age of 20–25 yr into ratios, were dealt with exactly as in Study I.

For purposes of correlation with ranked variables, the variable of U:P adjectives relating to the age of 20–25 yr was transformed into a five-rank variable according to percentile ranking.

Results

The distributions of answers from adults (Table 5) were compared with those from the children's groups (Tables 2, 3). In every case where the differences among the children's groups were non-significant, the comparison between the adult and children's groups was made in terms of the average value of all children's groups.

Table 5. *Ratio between U and P memories and between U and P adjectives relating to the period of* 20–25 *yr of age in the adult group*

	Mean ratio							
	U > P		U < P		U = P		Total U:P	
	n	U:P	*n*	U:P	*n*	U:P	*n*	U:P
Memories	22	2·28	10	2·11	3	1	35	1·69
Adjectives	10	3·77	21	4·10	4	1	35	1·39

There were no significant differences in any major respect, for the ratios of U:P memories ($z = 0{\cdot}25$, n.s.); for the distribution of subjects as to whether they had U > P, U < P or U = P memories ($\chi^2 = 2{\cdot}80$, n.s.); for the ratios of memories in the subgroups of subjects with U > P or U < P memories ($z = 0{\cdot}91$, n.s.); for the percentages of total U (58·11 per cent) and P (41·89 per cent) memories (for comparison, see Table 1) (CR = 1·35, n.s.); and for the ratio of U:P adjectives ($z = 0{\cdot}725$, n.s.). Concerning the latter point, it is nevertheless evident that adults' evaluation of the age 20–25 yr tends to be more negative than children's evaluation of childhood. Again, it is remarkable that only 3 per cent of the related U memories referred to possible traumas, such as death of a dear person, a dangerous illness or other injurious events.

In view of the above, it is not surprising that the results in the adult group fully support those in the children's groups (aim 1), i.e. the correlations—(*a*) between the overall evaluations of happiness at the age of 20–25 yr and the U:P ratios of memories from that period ($r = 0{\cdot}21$), and (*b*) between the overall evaluation of happiness at the age of 20–25 yr and the wish to return to that period (contingency coefficient 0·23)—did not reach significance.

Likewise, the correlation between the overall evaluation of happiness in childhood and the wish to return to childhood was in the adult group, as in the children's groups, non-significant (aim 2). The wish to return to childhood seems at first to be more pronounced than in any of the children's groups (compare Tables 6, 4), but this impression is created only by there being in the children's groups more expressions of the wish accompanied by reservations. When the negative answers are compared, the adult group does not differ significantly from the 12.1–14 yr group.

Analysis of results relating to the hypotheses of Study II (aim 3) shows that hypothesis 3*a*, which postulated a generalized attitude in regard to going back in time, was only partly proved. The relation between the wish to return to childhood and the wish to return to the age of 20–25 yr was non-significant for the whole group but proved significant only for the subjects who gave negative answers ($P = 0{\cdot}02$, one-tailed).

Table 6. *Distribution of answers of adults in percentages in regard to wish to return in age*

	Yes	No	Yes with reservations
Returning to childhood	35·71	57·14	5·71
Returning to 20–25 yr of age	31·42	62·85	5·75

The results give even less support to hypothesis 3*b*, according to which the four factors—evaluation of happiness in personal childhood, childhood in general, age of 20–25 yr and of humanity in the past—should be correlated. Only one of the six possible correlations proved significant: that between the evaluation of happiness in personal childhood and in childhood in general (contingency coefficient $C = 0{\cdot}548$; $P < 0{\cdot}01$; maximum value of $C = 0{\cdot}816$). It is important to note that 78·5 per cent of the subjects whose rankings of happiness in personal childhood and childhood in general were not identical ranked happiness of childhood in general higher than that of personal childhood.

In order to establish in how far the four variables relating to the evaluation of happiness in the past (aim 3*b*) are independent of the two variables relating to the wish to return back to the past (aim 3*a*) correlations were computed between the former and latter variables, but none proved significant. Neither were significant the correlations between any of the four variables of evaluation of happiness in the past and the variable of degree of incongruence between the wish to return back to childhood and the wish to return to the age of 20–25 yr.

DISCUSSION

The main findings of Studies I and II are as follows. (*a*) In regard to past (but not recent) periods of life children and adults recall relatively more memories which they judge as U than as P. The ratio of U:P memories is stable across the studied age ranges and equals on the average 2·07. (*b*) The evaluative attitude to the past period as a whole tends to be more positive than negative. The ratio of U:P descriptive adjectives tends also to be stable across the studied age ranges, and equals on the average 0·63. (*c*) The evaluative attitude towards a past period is uncorrelated with the relative amounts of U and P memories recalled from that past period, except in children 8.1–10 yr old in regard to their childhood. (*d*) The wish to return to a past period is uncorrelated with the relative amounts of U and P memories recalled from that period. (*e*) The evaluative attitude towards a past period is uncorrelated with the wish to return to that period.

These findings point to the existence of three types of dissonances, i.e. those created by the uncorrelatedness of the three attitude variables—judgement of memories recalled from a past period as P or U, overall evaluation of the past period and the wish to return to that period. These findings are puzzling for three reasons. First, because the dissonances are not resolved with time, as the hypotheses led us to expect. Secondly, because the dissonances are not specific for the relation to one particular life period but at least for two, if not for more. Thirdly, because of the stability and nature of the relevant measures of the attitudes across ages (children and adults) and across past life periods (childhood and the period of 20–25 yr).

Some of the results of Study II allow at least the hypothesis that the existence of the named dissonances as well as their non-relatedness to particular life periods is due to the embeddedness of the measured variables in the framework of more general conceptual structures. It may be assumed that within the structures themselves congruence between the constituent elements may be the rule, at the expense of possible incongruities which might result between elements of these structures and elements outside it, namely, of other structures.

The slight evidence for this hypothesis afforded by these findings with adults is, first, the correlation between the evaluative attitudes to personal childhood and to childhood in general, and, secondly, the correlation between lack of wish to return to childhood and lack of wish to return to the age of 20–25 yr. While the second finding only vaguely justifies the assumption of a generalized attitude towards returning to the past, the first finding suggests more clearly the possibility that the evaluate attitude, at least towards childhood, is not changed so as to accord with one's memories, since such a change would involve one in a new dissonance, i.e. between the happiness evaluation in one's childhood and the conception of happiness one has about childhood in general. Further support for this view is afforded by the tendency to judge the happiness of childhood in general as somewhat greater than that of personal childhood, i.e. the judgement of childhood in general may be assumed to elevate the judgement of happiness in personal childhood. Yet this does not explain why judgements in regard to the P or U nature of the memories are not changed to accord with the evaluative attitude. The argument that memories may be too important or lively to be changed does not apply, since the change should be effected in this case only in regard to the present judgements of the U or P nature of the memories and not to the memories themselves. Moreover, there is also the secondary problem of why the general evaluation of happiness in one period is uncorrelated with the evaluation in another period and in the past in general.

There remains the puzzle of the stability and nature of the relevant measures of the attitudes, particularly of the ratios of U and P memories and of the U and P happiness adjectives, across ages and past periods. The stability of these measures, which are responsible for the resulting dissonances, points to the conclusion that we might be dealing here with general tendencies characteristic of evaluative judgement of events and situations. The fact that the greatest majority of U memories related to regular repeated occurrences of usual events or situations excludes attribution of the negative balance of the U:P memories ratio to the greater impressiveness of traumatic events. In this context, it is tempting to hypothesize that similar effects might result when the object of judgement is changed to be a present person, object or situation. Namely, the overall attitude could then be regarded as a kind of evaluative label, which, perhaps because of conventions, tends to be positive, while the U:P ratio would represent the nature of information about the object of judgement, which might very well be independent of the label.

However that may be, the findings of these studies point to the need to test the various congruence models in the framework of cognitions referring to domains of life more vital to subjects than those created in the laboratory. This testing might broaden the scope of the models so as to include specification of those conditions which not only allow for the existence of steady dissonances but even make them imperative in view of the complexity of internal and external reality.

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INDIVIDUAL DIFFERENCES IN THE REPORTED PERSISTENCE OF VISUAL AND LABYRINTHINE AFTER-SENSATIONS, AND OF EXPONENTIALLY DECAYING VISUAL AND AUDITORY SIGNALS

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Two experiments are reported in which positive and significant correlations were found between the reported persistence of visual and post-rotatory labyrinthine after-sensations. In addition, the persistence of these after-sensations was also positively and significantly correlated with the time for which exponentially decaying, auditory and visual signals were discriminable against a fixed background signal. On the basis of these findings, it was suggested that the relatively stable interpersonal variation observed within these tasks was largely due to the function of a 'judgement' or 'discrimination' mechanism within the central nervous system which was common to several sensory projections.

Two separate arguments lead to the hypothesis that individuals who consistently report long after-sensations of seen motion will also report relatively long sensations of bodily rotation following impulsive stimulation of the non-auditory labyrinth.

The first argument rests on the observation that both visual and labyrinthine after-sensations decay exponentially (Taylor, 1963; van Egmond *et al.*, 1949). Thus the act of judging the cessation of apparent motion is likely to be similar, following either visual or vestibular stimulation, and in both the judgement is rendered difficult by the ambiguous nature of the end-point. If it is assumed that there are characteristic individual differences in the criteria used to decide that an exponentially decaying signal can no longer be detected, then it can be argued that these differences will reveal themselves in a positive relationship between the persistence of the two types of after-sensation.

The second argument, which leads to the same predicted relationship, is derived from the observation that persistence of both visual (Reason, 1966) and labyrinthine after-sensations (van Egmond *et al.*, 1948) increases as a logarithmic function of the stimulus. This suggests that there are common features in the way signals are transduced in the two sensory systems. A number of authors (Eysenck, 1955; Petrie *et al.*, 1963; Gray, 1964) have suggested that individuals may vary characteristically in the magnitude of the sensory response which is evoked by a particular level of environmental stimulation. If this is so, then an idiosyncratically large stimulus–response relationship may be reflected by relatively long after-sensations in both sensory modalities.

These two possible sources of interpersonal variation, although presented as alternatives, need not be mutually exclusive. It seems probable that the reported persistence of either type of after-sensation will be a function of both *criterion* and

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sensory response variation. Clearly, however, both arguments assume that the source of the interpersonal variation, for both visual and labyrinthine after-sensations, is central in origin.

EXPERIMENT I

In this preliminary experiment, the individual differences observed in reporting the persistence of three types of visual after-sensation, namely the spiral, spokes, and waterfall illusions, were compared with those found in the persistence of labyrinthine after-sensations. On the basis of the arguments set out above, it was hypothesized that there would be a significant concordance between the reported persistence of all four types of after-sensation, irrespective of the sensory modality.

METHOD

Tests and procedure

(i) *Visual after-sensations.* The stimulus conditions for the spiral after-effect (SAE) consisted of a four-throw, 180°, logarithmic spiral on a disk 20 cm in diameter. The inducing stimulus for the spokes after-effect consisted of a disk 20 cm in diameter bearing 18 alternate black and white sectors. The waterfall illusion was induced by the movement, around horizontal axes, of a continuous belt of matt photographic paper bearing alternate black and white stripes, each 1·3 cm in width. Only an area 25 × 13 cm was exposed to the subject, the remainder being masked by black art paper. For all three illusions, the inducing stimulus was situated at a distance of 1·7 m from the subject, and illuminated by a 100 W bulb situated 21 cm above and 16 cm in front of the stimulus area. The speed of the stimulus movement was kept constant at 600°/sec for the SAE, 180°/sec for the spokes illusion, and 2·5 cm/sec (equivalent to 0·9°/sec) for the waterfall illusion. For the SAE and spokes illusions, a fixation point was provided by a brass boss in the centre of the disk. For the waterfall illusion, a stationary fixation point was provided by a small cardboard disk supported in the centre of the stimulus area.

The same four values of induction period were used for all three visual after-sensations, namely 15, 30, 60 and 120 sec. A different randomized order was used for each subject within each test. The order of these tests was also randomized. The subjects were instructed to say 'now' when they could no longer detect the apparent negative motion. The persistence times were recorded to the nearest 0·2 sec, using a stop-watch. For each subject, one after-sensation persistence value was recorded for each induction period.

(ii) *Labyrinthine after-sensations.* The testing situation has been fully described elsewhere (Benson, 1962). The subject sat in a chair mounted on a turntable which was operated by a servo-controlled electric motor. The subject's head was supported in the vertical position close to the axis of rotation with the aid of a dental bite. Opaque goggles obscured all visual reference, and he was also instructed to keep his eyes closed.

Illusory sensations of turning were induced by the sudden stopping of the turntable (i.e. by an angular impulse) after rotating at constant velocity for at least 1 min. Acceleration from rest to constant speed occurred at the near threshold rate of 1°/sec^2. Four turntable speeds were employed, namely 7·5°, 15°, 30° and 60°/sec with rotation in clockwise and anticlockwise directions. For each subject these eight stimuli were presented in a different random order.

The measures. For each subject, a total persistence score was obtained for each of the visual illusions by summing the after-sensation durations evoked by the four values of objective stimulation. With the vestibular sensations, the mean of the two directions of rotation was employed in the calculation of the total persistence score.

Subjects. All four after-sensation tests were administered to 13 men whose average age was 21 yr (range 17–26 yr). No subjects had had any previous experience of these phenomena. All the subjects possessed uncorrected vision and were without apparent otological defect.

RESULTS

Spearman rank correlation coefficients (r_s) were calculated between the total persistence scores for the four types of apparent motion. The results have been set out as a matrix in Table 1.

All the correlations were positive and were significant at the $P < 0{\cdot}01$ level (one-tailed test). The close association between the durations of the three visual after-effects was perhaps to be expected because they shared the same sense modality and differed only in the form of the kinetic stimulus. However, the demonstration of a significant correlation between labyrinthine after-sensation times and all the visual illusions lends support to the hypothesis that there is a positive relationship between the illusions of apparent motion in the visual and vestibular modalities. Whereas the vestibular after-sensation is primarily determined by the physical properties of the end-organ, the visual after-effect is of neural origin, thus it can be concluded that the underlying cause of the interpersonal variation, whether it be *criterion* or *sensory response* variation, is likely to be determined centrally rather than peripherally.

Table 1. *The relationships between the four after-sensation persistence measures* ($n = 13$)

	1	2	3	4
(1) Spiral	.			
(2) 'Spokes'	0·91**	.		
(3) 'Waterfall'	0·87**	0·85**	.	
(4) Labyrinth	0·78**	0·71**	0·75**	.

** $P < 0{\cdot}01$ (one-tailed test).

EXPERIMENT II

A second experiment on a larger number of subjects was performed to confirm the observed relationship between spiral and labyrinthine after-sensation persistence. In addition to these two kinetic tests, two other tasks were introduced, one visual and the other auditory, in an attempt to elucidate the nature of the stable and apparently unitary source of the interpersonal variation underlying the observed individual differences in visual and labyrinthine after-sensations.

The additional visual and auditory tasks were designed to simulate, as closely as possible, the conditions associated with the judgement of the cessation of illusory sensations of apparent motion. In both of these tasks the subject was required to report when he could no longer detect the presence of a signal which decayed exponentially. In the auditory task he was presented with a pure tone which decayed in the presence of a fixed intensity of white noise; in the visual task, the brightness of a spot of light, in the centre of a disk of constant luminance, decayed exponentially. The important difference between these tasks and those employed in the first experiment was that the sensations were veridical and hence time-dependent on measurable physical events.

In the present experiment an attempt was made to obtain more information about the intensity and time course of the afferent vestibular signal which engendered the sensations of turning by recording the post-rotational nystagmic response. The slow phase velocity of the nystagmus was measured as this component is of physiological importance, and is considered (Lorente de No, 1933) to be closely related to the neural signal from the ampullary receptors of the stimulated canals.

METHOD

Tests and procedure

(i) *Motion after-effects.* The test situation and procedure for the SAE was identical to that used in Expt I. The labyrinthine after-sensations were also elicited in a similar manner with the exception that, in the present experiment, provision was made for the recording of the nystagmic eye-movements following the stopping stimuli.

(ii) *Nystagmus.* The conventional technique of electronystagmography (Aschan *et al.*, 1956; Benson, 1962) was used to record the post-rotatory horizontal eye-movements. For each subject the nystagmus engendered by the two 60°/sec impulses (clockwise and anticlockwise) was measured, and from the plot of log slow-phase angular velocity against time after stopping, the time constant of decay was determined.

(iii) *The auditory signal task.* The apparatus and testing procedure have been described elsewhere (Reason, 1964). The apparatus generated an 860 Hz pure tone, the intensity of which decayed exponentially with a time constant of 16 sec. The pure tone was mixed with a fixed-intensity random noise signal (20–20 kHz) so that when the experimenter operated a switch the subject heard both the decaying signal tone and the fixed masking noise through earphones. He was instructed to indicate when he could no longer detect the tone against the masking noise.

Four values of initial signal intensity were used, equivalent to 1, 2, 4 and 8 V (approximately 64, 70, 76, and 82 db re: 0·0002 dyne/cm^2 at the earphones, respectively). The noise intensity was approximately 44 db. Each value of initial signal intensity was presented to the subject four times from a randomized design. Each subject made 16 judgements in all. The testing was carried out in a small, quiet room, the subject being separated from the experimenter and the test apparatus by a screen.

(iv) *The visual signal task.* The subject sat in a darkened room 1·2 m in front of a ground-glass screen, 12 cm in diameter. This was evenly illuminated by four neon bulbs, placed behind the screen, to give a constant luminance of 14 cd/m^2. In the centre of the screen an image (8 mm in diameter) of the circular cathode of a small neon bulb was projected. The bulb was energized by a rectangular waveform generator with a pulse-repetition frequency of 1000 Hz; pulse width was modulated so that the brightness of the contrast spot decayed exponentially when a capacitor was discharged. The initial luminance of the central 'signal' spot was 45 cd/m^2 and the time constant of decay 16 sec.

The subject's task was simply to report when he could no longer discriminate the signal spot on the illuminated screen. Each subject made four consecutive judgements of the same visual stimulus.

The measures. For the visual and labyrinthine after-sensations, a total persistence score was obtained for each subject in the same manner as in Expt I. In the case of the auditory signal task, a total judgement time score was obtained for each subject by summing the mean judgement times over each of the four levels of initial signal intensity. In the visual task, the total judgement time score was obtained by summing the four judgement time values for each subject. The derivation of the nystagmus decay has already been described.

Subjects. Forty-eight male apprentices from the Royal Aircraft Establishment were used. Their ages ranged from 16 to 20 yr, with a mean of 17 yr.

RESULTS

The principal findings of this experiment are set out in the form of a correlation matrix (Table 2). The correlation coefficients obtained from this experiment confirmed that spiral and labyrinthine after-sensations were positively and significantly related. In addition, there were positive and significant correlations between the total persistence of these after-sensations and the total judgement times obtained from the auditory and visual signal tasks. Thus there was a significant concordance between all of the four measures requiring the judgement of the *disappearance* of an exponentially decaying signal. There was little difference between the degree of association observed between these different test situations. The nystagmus time constants were positively and significantly related to the reported persistence of the labyrinthine after-sensations, but were unrelated to the other measures.

Table 2. *Product-moment correlation coefficients between the experimental measures* ($n = 48$)

	1	2	3	4	5
(1) SAE persistence	.				
(2) Labyrinth persistence	0·43**	.			
(3) Auditory signal persistence	0·41**	0·42**	.		
(4) Visual signal persistence	0·39**	0·37**	0·38**	.	
(5) Nystagmus time constant	−0·08	0·34*	0·09	0·08	.

** $P < 0\ 01$, * $P < 0{\cdot}05$ (two-tailed test).

DISCUSSION

In a variety of tasks, in which subjects were required to judge when an exponentially decaying signal disappeared, it would appear that the factor responsible for the inter-subject variability was not primarily dependent upon the modality of the stimulus, nor upon the veracity of the sensory information, but rather upon the function of a *judgement* or *discrimination* mechanism within the central nervous system which is common to several sensory projections. The relatively low correlation between nystagmus time constant and labyrinthine after-sensation persistence suggested that this (theoretically expected) relationship was attenuated by variation in the end-point decision criteria. But the fact that these two measures were positively and significantly correlated at all prohibits us from accepting these judgemental differences as the sole source of variation. Clearly, differences in the level of the underlying neural activity were also involved in the determination of after-sensation persistence.

Two possible sources of interpersonal variation have been postulated, but it still remains to determine which, within the individual, is the more stable. It is also necessary to evaluate their relative contributions to the observed individual differences.

Finally, it must be noted that, since the completion of these experiments, evidence has become available which has a close bearing on the findings reported here. In a recent study, Nilsson & Henriksson (1967) found a positive and significant relationship between the persistence of the SAE and that of post-rotatory apparent movements of a light spot—the oculogyral illusion (OGI). It is generally accepted that the OGI and the illusion of bodily rotation following an impulsive stimulus are both time-dependent upon the same vestibular events. Consequently this finding can be taken as additional support for our contention that consistent individual differences observed in reporting the persistence of spiral and labyrinthine after-sensations emanate from the same central source.

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A NOTE ON A MACHINE THAT 'LEARNS' RULES

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A machine (C) is described which will learn to mimic *creatively* the behaviour of another machine (M) in this sense: after a certain number of 'observations' of M, C will begin to produce sequences of symbols not only like those that M produced but also like those that M might have produced but did not in the circumstances do so. If we say that M's behaviour is *as if* it were regulated by *rules* then we could say that C has learnt them. For human beings, postulation of an ability of this type seems to be necessary to account for the learning of language and for other social activities where behaviour is regulated by conventions. 'Reinforcement theory' is unable to account for this type of learning. C carries out its task without any 'feedback' or 'reward' from M; indeed, the only function for 'motivational' factors is to direct C's 'attention' from observing M to observing another machine or to cut off C's input entirely. If C were an organism it would need a set of primitive perceptual categories and an innately active nervous system to carry out certain restructuring operations on the knowledge that it acquires.

Rule-regulated behaviour: general considerations

In an attempt to elucidate the idea of a social science Winch (1958) focuses attention on 'rule-regulated' behaviour. He argues that all 'rules' are characteristically *social*, for it only makes sense for someone to say that he is following a rule if it is in principle possible for someone else to discover it. Put another way, this means that when we say that a *mistake* has occurred, we are in fact saying a contravention of what is established as correct has occurred, and a mistake must be *recognizable* as such a contravention. Here we are appealing to something like a 'rule' in a social context.

A rule (or a rule-system, a hierarchy of rules) indicates how one action should follow from another. Somebody having to carry out the *same activity* as someone else could do something *different* from what he was originally shown, but which *in relation to the rule-system* counted as going on in the *same* way. This is a problem which arises over and over again in psychology: What counts as doing the *same thing* even when it is quite impossible to produce any criteria of physical correspondence for the observed activities?

How in practice can rules be discovered solely from the observations of rule-governed behaviour? This problem is one of crucial importance for human psychology, for the most distinctly human characteristic is the ability to use language, and this is an outstanding example of rule-regulated activity. Every normal child learns not only the 'rules' of his language but many other types of conduct where there seem to be criteria for what is 'received', 'acceptable' or 'recognized'. The 'rules' are not necessarily explicit; indeed they are usually not so. There need only be implicit agreement upon what conforms to the 'rule' and what must count as a mistake and the 'rule' is induced from examples of its use. Any systematic account of human behaviour and development must account for this ability.

More specific considerations

There is no doubt that in learning a natural language a child learns how to *create* (Chomsky, 1959) his words and sentences when necessary. It is quite impossible for him to learn complete individual sentences to cope with every occasion (Miller *et al.*, 1960, p. 146).

To account for language learning, theories of imitation have been proposed. One such is Mowrer's (1960). It is cast in terms of the principles of secondary reinforcement derived from 'learning theory'. However, even if Mowrer's theory were successful in its original aim of accounting for mimicking in mynas (which it does not seem to be—see Foss, 1964), it would still be inadequate to account for the creative aspect of linguistic activity. In such activity, component forms are not freely combined, strict 'privileges of occurrence' are respected, and the difficulty lies in accounting for these.

Imitation does not occur in all animals (Thorndike, 1898), but when it does, it appears to be such an automatic process that it is difficult to discover any ways of manipulating it. Furthermore, although it is known to play a great part in language development (Lenneberg, 1964), even when a child does not overtly imitate because he is dumb and cannot, he can still come to understand language without any special training (Lenneberg, 1962). Some internal organizing processes seem to be at work on the material that the child has derived from its environment. Unchanged, this material would only be adequate as a basis for *direct* imitation; in its restructured form it must provide a basis for *creative* mimicking.

What is described below is how a machine might be made to carry out an elementary form of this process. The intention is not to provide a psychological theory of imitation as such: the machine (C) described is already able to imitate directly the behaviour of some other machine (M) that it has just 'observed'. What C does is not only to behave as it observed M behaving, but also to behave as M could have gone on to behave but did not in fact do so while C was 'observing' it. It is as if M's behaviour was regulated by rules and C had extracted these to regulate its own subsequent behaviour (cf. Craik, 1948, on 'internal models').

C behaves in a manner similar to a child learning the 'production rules' of a generative phonology. The problem of such learning is far too complex to be dealt with in a short note. The aim of this note is to exhibit the feasibility and properties of a very simple rule-learning machine, and to discuss very briefly some of its psychological and neurological implications.

How C works

The initial relation of C to M is shown in Fig. 1. Each machine has one input and one output. M is drawn just like C as it may in fact be similar to C, except that it is already in the position of containing a rule-system or 'model' derived from its 'observations' of some other machine. C 'observes' M via the connexion 'as'. Assume that M outputs certain sequences of distinct components (for example, decimal digits), arranged in certain patterns; for example,

0/P:1...1234231... and 0/P:2...1112311...

Not every combination of digits appears in the sequences; only certain digits may follow a specific digit. Thus M operates as if it were following a set of 'rules'. C must arrange itself so that it outputs patterns which 'conform' to these 'rules'. Initially each new symbol that M emits is represented inside C by a distinctly labelled state. Thus C might represent the sequence 0/P: 1 as in Fig. 2(*a*).

If C now stored this representation it could use it as a 'model' and create a string of symbols just like it when necessary. However, the sequences of symbols ... 2 3 ... is represented twice. It is uneconomical to store the *same* thing twice—can this redundancy be reduced? Furthermore, it is quite likely that M was in the same internal state each time it produced a '2' or a '3'; is there any way of representing that fact? It happens that the same rearrangement process operating on representation (1) will solve both problems.

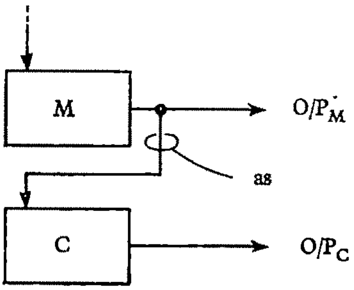
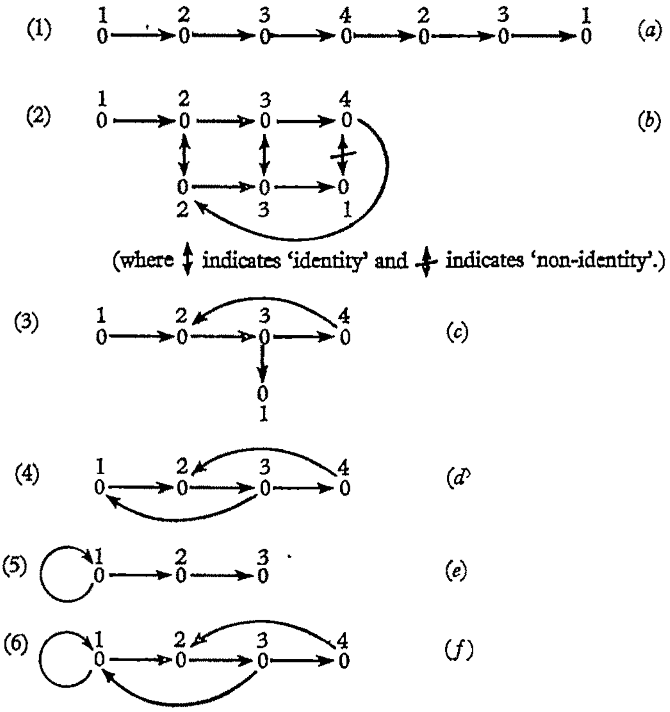


Fig. 1



(where ↕ indicates 'identity' and ↮ indicates 'non-identity'.)

Fig. 2

Representation (1) may be rearranged as in Fig. 2(*b*) to indicate the possibility that the same internal states of M were involved when a '2' and a '3' were produced. (This is equivalent to C making a tentative 'assumption'.) The two pairs of states 'assumed' identical can now be merged to produce the 'model' as in Fig. 2(*c*) (in more complex machines than C described here various tests have to be satisfied before the merging may take place, and the 'assumption' must be relinquished if the test is not passed). Repeating the process again on representation (3) produces (4) of Fig. 2(*d*). Carrying out the same process on 0/P: 2 gives representation (5) (Fig. 2(*e*)) and this can be combined with (4) to give (6) (Fig. 2(*f*)). If C now used (6) it could produce an infinity of strings made up of the specific patterns of 1's, 2's, 3's and 4's that (6) allows. Representation (6) gives C's 'model' of M to date; it may or may not be *complete*. (The conditions under which the model can be known to be complete, although they can be stated, are too complex to be discussed here.) However, whether (6) is a complete model of M or not, C is now in a relatively powerful position, for if 1, 2, 3 and 4 are the only digits that M does produce, then C is now able to construct an infinity of patterns from them that will all be *like patterns of* M. If any other patterns of outputs from M do turn up they can be incorporated quite easily by the same process into C's 'model'.

A distinctive feature of language, though, is that not all possible combinations of component forms do occur; only a limited set does so (this is one reason why Markov processes with probabilistic transitions are quite definitely unsuitable to characterize the structure of language, even though the majority of *n*th-order (with large *n*) approximations to English may seem relatively grammatical). Thus if M is actually following rules (it may not be doing so), and the digit sequences do not exemplify all possible combinations of digits, there will come a time when every sequence of digits that C 'observes' is represented by some pathway in the 'model'. When this point is reached (and for certain machines there are ways for deciding this) the connexion 'as' (for *a*pron *s*trings) can be severed. C can now behave *like* M.

In (6), nodes 1 and 3 have two arrows leading from them, and if C were using (6) to mimic M there would be no clear decision procedure for selecting which of the arrows to follow. If an arbitrary but systematic procedure (for example, always take path *not* used last time) were followed, then the model is said to be *non-deterministic*, but if probabilities are assigned to the paths which are then chosen at random it is said to be *probabilistic*. This is a complex point (see Rabin & Scott, 1959, for a discussion). It might be said that to mimic M properly, C would have to 'weight' the alternative paths precisely as M does (but such questions as these remain to be decided). However, the non-Markovian character of natural languages would seem to exclude the need for probabilistic models.

This completes the outline of C and the processes that it incorporates.

Implications for psychology

The first point to make is that C 'mimics' M quite automatically; there is no 'feedback' or 'reward' from M. The next is that the only 'judgement' that C need initially be capable of is that of 'identical or different'. Later, of course, C could use (6) to 'recognize' whether a string of digits is in fact one of M's or not. Finally, the 'learning' process will only stop when either C's model is complete or the link between M and C is broken. Analogously, it may be said C will only stop learning about M if its 'attention' is redirected.

Any neural embodiment of C would need to possess two features. First, it would require the capacity to represent within itself spatial and temporal sequences of events. There have been a number of theories as to how this might be done, including the 'physiological isomorphism' of the Gestalt theorists (e.g. Köhler, 1929), the 'cell assemblies' and 'phase sequencies' of Hebb (1949), and the units of Hubel & Wiesel (1962) (for vision) and of Frishkopf & Goldstein (1963) (for audition). All that is required is a representation in terms of discrete units (not necessarily neuronal ones); recent evidence seems to support this assumption. Secondly, any neural embodiment of C would require the capacity to perform *restructuring operations* upon its own representations of sequences of external events; there is no evidence directly bearing on this, and it is hardly worth attempting to infer from indirect evidence.

As many animals learn but do not creatively mimic, this process clearly cannot supplant 'reinforcement theory'. However, it does not seem too unreasonable to suppose that, as the evolutionarily highest organisms, we incorporate not only advanced mechanisms for learning but also all those available to organisms less elaborate than ourselves.

Applications of the machine C to the language situation cannot be discussed here. The machine which has been described is extremely simplified and many background assumptions and technical details have been ignored so as not to obscure the main point. The aim has been solely to outline a mechanical process that could in some way 'mimic productively' in the way demanded by the facts of language development. If this note can help to legitimize theories of human behaviour couched in terms of 'rule-regulated' processes, it will have served its purpose.

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PUBLICATIONS REVIEWED

Interpersonal Styles and Group Development: An Analysis of the Member–Leader Relationship. By Richard D. Mann in collaboration with Graham J. Gibbard and John J. Hartman. New York, London and Sidney: Wiley. 1967. Pp. xi + 308. 62*s*.

The authors criticize experimental research on small social groups on the grounds that 'potential consumers of such work feel that...[it]...is constantly asking the wrong questions in the wrong ways, and that it is at best irrelevant and at worst inaccurate and misleading'. They feel that we need to take a fresh, clinical, look at group processes, and at groups that endure over a period of time. They provide us therefore with a detailed study of four self-analytic groups. As a source of empirical data T—groups and therapy groups are no more satisfactory than laboratory groups—they are largely the product of a very unusual style of leader behaviour, and what goes on is entirely unlike anything else, except perhaps plays like '*Who's afraid of Virginia Woolf*'. The therapeutic value of these groups is also very questionable in view of the extreme emotional disturbance produced in some of the participants; and it is arguable that interpersonal feelings are better expressed by means of non-verbal communication.

Bales and his colleagues introduced a course at Harvard known as 'Soc Rel 120', in which groups of 20–30 students go through what is very similar to a T–group procedure and for which credits are given. Richard Mann and his two collaborators have studied four of these groups. They give a lot of verbatim material and have adopted a 'clinical' or case-study approach to these particular groups. They have also used quantitative procedures and in a rather original manner. Clinically derived categories for interaction-recording were used, and the data factor analysed. Six factors of member-to-leader behaviour were found, together with the ways in which these particular leaders reacted. Of particular interest is the analysis of role-differentiation; six roles appeared in all four groups: hero, moralistic resister, paranoid resister, distressed females, sexual scapegoat and male and female enacters. This is quite the most interesting study of this topic that I have come across. Analysis of group development shows six phases—two more than Tuckman's stages, adding 'premature enactment' and death of group.

It is becoming increasingly clear that different kinds of group function differently. It follows that the strategy of isolating certain abstract elements, embodying them in experiments, and hoping that the results are universally true, may not pay off. The next stage in small-group research should perhaps be to study real groups of adolescents, of workers, of scientists, of directors, and members of families, to find out the actual motivations, environmental pressures and constraints and the particular interaction processes that take place.

MICHAEL ARGYLE

Progress in Experimental Personality Research. Volume 3. Edited by Brendan A. Maher. New York and London: Academic Press. 1966. Pp. xii + 319. 76*s*.

This book is a further volume in what so far has been a useful series. It is, however, perhaps worth while at this time to consider how valuable this type of book is in comparison to journal reviews on the one hand and to normal single author books on the other. Perhaps the main disadvantage of the collective book is that some readers may not want to pay in this instance 76*s*. (twice the cost of volume 1) for what may be only a single chapter which interests them. However, if, as seems to be the case, this may be the only way for economic reasons to publish papers of monograph length the enterprise must be considered worth while. With the spate of journal articles at the present rate of flow it becomes almost impossible to keep up with particular interests let alone to attempt to maintain some breadth of knowledge in the subject. It is as an aid to the 'up-dating' process that this 'Progress' series has become most valuable, and this present volume is no exception. Material is presented which would be insufficiently covered in a short paper and which would have to be tediously stretched to fill a book.

The first chapter by Gottesman and Shields presents a review of twin studies in schizophrenia from 1928 to the present. Tables of data have been given which have been gleaned from many sources and the authors give a very complete picture of the field, which in spite of the variability in the data from different workers leaves little doubt about the genetic contribution to the disease. The second chapter, by Mischel, is a good example of an experimental approach to differences in a particular aspect of personality, namely ability to wait for reward. A criticism which might be made is that the field of study and discussion tend to be somewhat circumscribed and consideration of the work in relation to other approaches to personality might be profitable.

The third chapter, by Simes, is entitled 'Actuarial Methods in Personality Assessment'. It gives a clean and detailed presentation of techniques which perhaps have to be defended less in this country than in the U.S., where 'there is a great deal of readily expressed doubt that actuarial methods of prediction can be successful because, unlike the clinician, they fail to consider either the subtleties of the test data or the uniqueness of human personality'.

The fourth chapter, by Steiner, presents an experimental approach to aspects of the problem of resolution of interpersonal disagreements and relates these findings to some standard personality variables.

The final chapter, by Austin Jones, is a very complete review of the work by the author and his colleagues showing that, when deprived of information (in the technical sense), the subject takes steps to remedy this deficit. The experiments discussed are models of successive examination of leads opened up by the studies which preceded them. Although not all chapters will intimately interest every reader there is almost certainly sufficient to make this a valuable collection to have on the shelf.

P. H. VENABLES

Psychologie der Sprache. By H. HÖRMANN Berlin, Heidelberg, New York: Springer-Verlag. 1967. Pp. x +395 $14.50.

Handbooks and treatises are out of favour nowadays. Change is, or progress seems, so rapid that the stationary observer has little to catch hold of. Science has no longer even a specious present. So it is hardly surprising that, apart from Kainz's unwieldy and already dated work launched in 1940 and still incomplete after four volumes, nothing covering 'psycholinguistics' or the psychology of language has appeared since Miller's *Language and Communication* in 1951. Professor Hörmann's solid volume is something of a surprise, and a welcome one.

The psychology of language has been stirred up during the past twenty years by a number of frequently conflicting incursions from information theory, statistical dissection, mathematical and unmathematical linguistics, and neuropsychology. None of these has set it on a single course. The resulting diversity is, as to quite a large part, fairly and sympathetically, if a little painstakingly, covered by Professor Hormann. The feature of this book which the average British or American reader of less than middle age may find novel is the effort to make evident the relation of contemporary notions to those current before the War.

Two introductory chapters briefly consider a number of views about the nature and origin of speech and its reationship to animal communication. These lead the author to define the field with which he is to concern himself in terms of a 'schema of speech events' which places strong emphasis on the encoding and decoding aspects of these events. This in turn calls for a definition of the elements involved and of the forms of their relationship. Gaining knowledge of the function and acquisition of such systems of relationship is, according to the author, the task of the psychology of speech. There might be different opinions about the adequacy of such a definition, but in the event Professor Hörmann does not burk so many issues as it might seem to allow him to. From this starting-point the course of a single chapter takes us from speech sounds to generative grammar by way of Martinet's 'levels' and Jakobson's 'distinctive features'. The following two chapters cover adequately what might now be referred to as the 'older' or 'proto-' psycholinguistics; that is to say, those parts whose background is information theory and the probability structure of language. All the essential theory and experiments are clearly brought out. Once again, however, it is apparent that to provide an elementary and common-sensical rationale for the Shannon entropy-function is extremely difficult if not impossible. In this particular instance Hörmann adduces von Weizsäcker's philosophical approach, but to some Anglo-Saxon readers at least, this may prove less than clarifying.

With the next two chapters on the phenomenology and the mechanism of verbal association, we move beyond the purely stochastic aspect of speech structure and Hörmann gives us an admirably complete and well-documented account. He then briefly delves back into the philosophical background of modern psycholinguistics before considering meaning as a field-relationship. This discussion, based largely on the empirical work of Deese, leads to mediation theories such as those of Bousfield and Osgood and so to the more starkly behaviourist views of Mowrer and Skinner, together with Chomksy's criticisms of them.

After a further interlude on sound symbolization, the character and the psychological implications, theoretical and experimental, of contemporary grammar are tackled. In this, the basic reasons for present-day enthusiasms for this approach, as against the older statistical and information theory ones, are not perhaps made as clear as they might be. Nevertheless Professor Hörmann usefully brings together for the student material which up to now has largely remained scattered in the journals. A full and equally valuable chapter on the developmental aspects of speech follows and the book closes with some consideration, not wholly abstract, of the influence of speech on the 'Weltansicht' (a word better left, as Professor Hörmann has left many English ones, untranslated) of man.

This massive work, based on a large erudition which reaches not only backwards but as near as possible up to the present moment, still inevitably raises questions as to what ought to be included in a book entitled *Sprachpsychologie*. His own choice has its merits and its disadvantages. Thus he follows out faithfully the line of thought which starts, on the one hand with the stochastic structure, and on the other with the behaviourist interpretation, of speech and language and leads on through the rather desperate and increasingly verbal remedies of mediation theory to the recent developments stemming from the neogrammarians. He shows himself alert to the hazards of blind empiricism, but his exploration of possible new theoretical ideas, which are badly enough needed, introduces perhaps more purely philosophical considerations than are strictly worth their place.

The introduction of the newer American studies in the genesis of language in children is admirable. But what is most disappointing is the almost total absence of any reference to the cerebral mechanisms of language. It can no longer be said of this field either that it consists entirely of a mouldering mass of conflicting clinical evidence and conjecture, nor that problems, methods and findings have no connexion with those in other areas of research of which Professor Hörmann gives so good an account. Contemporary neuropsychological study of brain-injured people is now at a level of exactitude quite different from that of the classical neurologist, and is both more extensive and intensive in the scope of its data. It is informed by ideas in keeping with, and in many instances derived from, those of current psycholinguistics. Experiment on normal subjects, on the other hand, is in certain cases approaching the point where it may allow more or less direct inferences regarding the functions of the brain in speech. One must express the earnest hope that in the next edition of his book, which will assuredly be called for, Professor Hörmann will add at least a chapter outlining these new developments. The material is ready to hand in a number of different journals.

In one, important, respect this book on language differs refreshingly from most of its predecessors in the German tongue. The language in which it is written is simple and easily comprehensible. Though it cannot cater for the needs of all students it should certainly be in every psychological and linguistic library.

R. C. OLDFIELD

Readings in Physiological Psychology. By J. R. STRANGE and R. FOSTER. California: Wadsworth. London: Prentice Hall International. 1966. Pp. 387. 40*s*.

This collection of thirty-three articles, most of which were published within the last decade, is intended to make important original research literature readily available for undergraduate reading. The selection covers the following fields of physiological psychology: nervous system (3 articles), vision and hearing (4), chemical and somesthetic senses (4), emotion (3), motivation (10), learning (5) and brain lesions (4). While the selection of articles in any book of readings inevitably reflects the personal preferences of the editors, the choice presented here would seem to offer something for most students in the field. To name a few authors only, this reviewer is particularly happy to find the articles by T. H. Bullock, W. P. Neff, R. Hernandez-Peon *et al.*, C. Pfaffmann, D. H. Funkenstein, F. A. Beach, D. S. Lehrman and J. Olds. If a minor criticism of this otherwise admirable collection can be made it is that no space has been found to include articles on the physiological analysis of orientation, rhythms, maturation and genetics.

Each section is briefly introduced by the salient points covered in the subsequent articles and questions are given to prime the student's interest. In addition, the book contains a lucidly written historical introduction, a small glossary of technical terms and a useful index for cross-reference. I have no doubt that the book will be widely used for the purpose it so well fulfils.

ULRICH WEIDMANN

Analyses of Concept Learning. Edited by H. J. KLAUSMEIER and C. W. HARRIS. New York and London: Academic Press. 1966. Pp. 272. 68*s*.

Reviewers often complain that books of symposium papers are uneven in quality; this one, however, is consistently mediocre.

Harré fanfares the book with a peal of platitudes. 'Some ideas are muddled, confused, unclear and misleading, and other ideas are ordered, clear and veridical', and while you are recovering from this he stuns you with: 'the question of which ideas are the clear ones and which are the muddled has always been a matter of dispute', and so forth. Merrifield reels off a new theory of intellectual structure involving distinctions between operations (cognitive, memorative, etc.), contents (figural, symbolic, etc.), products (system, class, etc.), and types of learning (signal, S.R., sequence, etc.). It is no better and no worse than any other scrambling together of intellectual liquorice allsorts. Underwood argues that concepts are learned in the context of their predictive utility in *life* and then offers paired associates as the material for the study of concept learning. Kagan's message cannot be heard through the din of his battle with the English language. In his first sentence he beats it to the floor with 'the componentry of human mentation' and stomps it repeatedly with phrases like 'the determinants of attentional involvement' and (in children) 'restless motoricity'. Bayley reviews rather than rethinks, but does highlight interesting material on the inadequacy of I.Q. metrics as predictors of concept learning in adults. Ausubel sways clear of the horns of clarity like a bold bullfighter, concluding with a dazzling verbal cape swirl—'Thus it is useful, I believe, to define potentially meaningful learning material in terms of nonarbitrary and substantive relatability to the learner's cognitive structure, and to think of the principal determinants of subject-matter learning in terms of various salient properties of the learner's existing body of knowledge as they are relevant to the learning task.' Suchman contributes the sort of model-building exercise in which terms like 'storage', 'mediation' and 'intake' are put into boxes and linked with arrows so that they make as much sense as they did before they were boxed and arrowed. Jensen, in discussing individual differences in concept learning, makes explicit the reductionistic assumptions that underly virtually every essay in the book. He treats concepts which have never been logically (as opposed to operationally) related as if one were a potential component of the other; quote: 'Since short term memory plays an important role in concept attainment, I would expect that the factors discovered in short term memory tasks will also account for some of the individual difference variance in concept attainment tasks.' This is like expecting a study of typography to contribute to an understanding of literary criticism.

All the authors seem unhappily aware that they are committed to talking about those 'higher processes' that psychologists always knew we should postpone indefinitely. The book is studded with massive assertions of the obvious about which, it seems, nothing can be done, e.g. (Kagan) 'Unlike rats in laboratories who learn mazes in order to get pellets of food or avoid painful electric shock, children work at cognitive tasks for a variety of more sophisticated goals'.

Yet if sixteen respected psychologists manage to miss the point it suggests that there was no point to be hit. It may be that no sense was talked about 'conceptual learning' because, as the area is defined, it makes no sense. It is the sort of arbitrarily defined focus which forces psychologists into one of two traps. They must either assemble a ramshackle theory of concept learning all by itself, or try to stuff concept learning down the unsuitable and distended gut of learning theory or some such. Until we have a coherent over-all framework for psychology we shall continue to pick up lay concepts like 'concept', define them every whichaway and suffer while trying to compound them into a psychology.

DON BANNISTER

Attention and Performance. Edited by A. F. SANDERS. Amsterdam: North-Holland. 1967. Pp. xii + 452. 120*s*.

Information processing models have become increasingly popular over the last ten to fifteen years as descriptions of the phenomena of attention, memory and reaction processes. Using the language of information theory, and more lately of signal-detection theory, research workers have refined and quantified the early concepts until there now exists a fairly large body of theory and results. As much of the stimulus for this work has been European, it is appropriate that this large-scale symposium on the information processing approach, or 'Human Performance Theory' as it is called here, should have been held at Driebergen, Holland. The meeting was organized by A. F. Sanders in August 1966, and he has edited the proceedings of the symposium to make up the present volume.

The area covered ranges from discussions of single-channel theory and studies of the psychological refractory period through studies of attention and reaction-time, their physiological correlates, short-term memory, eye movements and visual search, to experiments on vigilance and long-term performance. Four of the sections have informative reviews of research in that area. There are many stimulating contributions and several authors give useful accounts of work from their laboratories over the last few years—I particularly liked the papers by Wilkinson and by Jerison. One theme which runs through several contributions is the possible sharing of central processor capacity and an interesting paper by Taylor, Lindsay and Forbes points out the possibility of quantifying this capacity in terms of d'^2.

This must have been an exciting symposium to attend but unfortunately good symposia do not necessarily make good books. The main weakness of the present volume is that, despite the evident efforts of the editor towards organization and coherence, it is not really a book so much as a bumper edition of a journal. Still, it is useful to have the papers collected together so that the current position in the various areas may be assessed. More serious is the fact that such a large book does not wear well in paper covers—could the publishers not have included hard covers for the rather daunting price? It is probably somewhat carping to complain of faulty English from a foreign editor but there are a good few misprints and unusual usages—did Taylor (p. 398), for instance, really mean to be so cutting as to say of a fellow contributor that he had 'developed an ingenuous theory' to account for his results?

Finally, I liked Rabbitt's comment after describing an experiment which had not fully confirmed his predictions: 'As functioning human beings we may be cheerful that the adaptive resources of the CNS are once again shown to be more complex than the models which we can propose, at present, to describe them.'

F. I. M. CRAIK

Organic Foundations of Animal Behavior. By JOSEPH ALTMAN. New York: Holt, Rinehart and Winston. 1966. Pp. xiv + 530. $12.95.

This text-book provides a good introduction to the biological foundations of psychology. Rather more than one half of the book is devoted to anatomy and physiology. The remainder deals with the interaction of the organism with its environment.

The chapters dealing with anatomy and physiology are clear and well integrated. They are not overloaded with detail; the most important facts have been selected and references are given to recent original papers. The illustrations are good and those dealing with the anatomy of the brain are particularly well chosen. There is also a chapter on techniques of studying brain function.

The section on organization of interaction with the environment outlines what is known of the neurophysiological basis of memory, learning and motivation and of the role of receptors and afferent mechanisms in the organization of behaviour. The subject of emotional behaviour is not covered under a separate heading. A final section deals with mechanisms for preservation of the individual (e.g. nutrition, sleep, hibernation, migration), propagation of the species, formation and maintenance of social groups, and mastering the environment (e.g. exploratory activities, play).

The most pleasing aspects of this book are that it is reasonably comprehensive and up to date for an introductory text and that it is more than a collection of facts: the author, as he states in the introduction, has attempted to 'present an integrated system of animal psychology employing the biological way of thinking and utilizing, wherever possible, biological principles of explanation'. One definite disadvantage of this attempt to integrate biology and psychology in a new way is that the author uses unusual terms which, even to the initiated, are obscure without reference to their Greek or Latin roots or to the meaning which the author gives them. For example, one chapter is called 'Paleocephalic control of recurrent catering activities: the affective integrative level'; another, 'Neencephalic control of singular instrumental activities: the cognitive integrative level'. These terms present only superficial difficulties to the reader, they do not interfere with the fundamental usefulness of this book, which can be recommended at least as a second choice among the text-books of physiological psychology now available.

DAPHNÉ JOYCE

Age and Function. By A. HERON and S. CHOWN. London: Churchill. 1967. Pp. x + 181. 45*s*.

Few empirical indices are so widely used, rigidly applied and generally deplored as the concept of a 'retiring age'. Heron and Chown describe an attempt to obtain an index of 'functional' as distinct from chronological age by subjecting an unusually large (540) and uniquely representative sample of people to twenty-five tests and measurements, and by using factor analytic techniques to map trends and association in the mass of data obtained.

The results of principal component analyses clearly disappoint the authors. Anthropometric measures (height, weight, biceps circumference) sensory acuity and physiological measures (blood-pressure, expiratory volume and grip strength) are significantly related to age but not to cognitive performance. Differences on personality tests (Heron I and II; Wesley rigidity) are age-independent. The most striking finding was that if non-verbal I.Q. is kept constant there is no age-related decrement on other measures of cognitive performance. Earlier findings of differential decrement on non-verbal I.Q. (marked loss) and verbal I.Q. (Mill-Hill Vocabulary, little loss) are again substantiated. An elegant technique allows confirmation of Raven's finding that matrices scores decline less with age in the high than in the low I.Q. brackets.

Many details testify to the discrepancy between chronological age and performance, and the presentation of 'performance profiles' of selected subjects convincingly underlines the diversity of the ageing process in different people. It is to the authors' credit that the data are presented with a clarity which convince the reader that they have not discovered a useful index of functional ageing. With restraint rare in gerontological studies they do not elaborate complex hypotheses on the basis of isolated relationships between indices. Occasionally this is a matter for some regret; pervasive and diverse age *vs.* sex interactions indicate a series of enticing research problems without providing points of orientation to them. An interesting discussion of the trade-off between speed and accuracy with advancing age is spoilt for the pedant because a major piece of evidence hinges on a comparison between speed-stressed and unstressed conditions of a digit coding test—which were always given in the same order to all subjects (p. 34).

The cumulative effect of the authors' clarity and honesty is to raise the question whether the conceptual returns of investigations of this size justify the great investments of time and skill necessary to carry them out. One may argue that the present study would have been better had some notable omissions (e.g. indices of psychomotor function) been repaired, and had the physiological indices been less crude and unrevealing. Does this mean that we should now carry out even more detailed studies, on bigger populations, with more and better equipment, with the support of more specialists and with the benefits of hindsight? On the whole, not.

Models of human performance are now surely sufficiently detailed to allow us to select constellations of indices within a particular function. Thus relationships between various indices within each of the areas of perception, memory or psychomotor performance have been mapped out in great detail for young adults. One can envisage very profitable studies in which factor analytic techniques became sensitive and subtle tools for assessing the validity of current functional models of performance in young people, and examining the adaptation of these systems to the processes of ageing.

In many ways this is an exemplary book. In a field bedevilled by sampling problems it is no minor bonus to British research workers to find a compendium of truly normative data against which smaller subject samples, in single experiments, can be compared. To many workers this may well serve as an outline course in how such investigations should be planned, how their results should be set out, and how they should be, internally, criticized. While keenly appreciating these virtues, other investigators will savour the moral that even with much more lavish technical resources, global, factor-analytic 'test-battery' approaches to ageing may well leave us, after exacting labour, moderately convinced of a number of things which we did not need to know.

PATRICK RABBITT

Early Behavior: Comparative and Developmental Approaches. Edited by HAROLD W. STEVENSON, ECKHARD H. HESS and HARRIET L. RHEINGOLD. New York: Wiley. 1967. Pp. ix + 303. 75*s*.

This book puts on record the results of two conferences held in 1963 and 1965 in an edited form. The conferences were organized by a committee, now dissolved, of the American Social Science Research Council, which had been set up to promote the study of 'comparative developmental behaviour'. In the introduction Hess expresses a hope for a new development of 'ethological psychology' which would edge away from the traditional American environmentalist outlook and show no fear of the concept of 'innate behaviour'. However, as Hess is aware, the contributions to this volume do not represent any such unity of approach. In fact, papers show a great diversity of viewpoints, which is all to the good.

Approximately half the contributions are concerned with young animals and half with human infants. The main animal topics are 'sexual imprinting' in altricial birds (Klinghammer), learning in subprimate mammals (B. A. Campbell), 'handedness' in cat and monkeys (Warren *et al.*), and social responsiveness in young chimpanzees (Mason). A more general discussion is found in a paper by Eibl-Eibesfeldt on concepts of ethology and their application to psychology. With regard to human infants the following receive special attention: Sucking and looking behaviour (Kessen), visual perception (Fantz, who also reports on species ranging down to the domestic chick), modes of learning (Lipsitt), and 'appetitional behaviour' (Papousek, working in the Pavlovian tradition). All the contributions are very valuable.

To review as a whole papers covering such a range of species and topics would be quite a formidable task. But this has been done at length in the final contribution—a most useful essay on 'a comparative psychology of development' by Rheingold. She first considers certain features of early behaviour, as surveyed and discussed in various papers, viz., responses that are present at birth, preferences for some stimuli over others, and the modifiability of behaviour, including decrement in response to repeated stimulation and more complex forms of learning early in life. Then, several types of research approach are distinguished; but it is clear that classifications of early-behaviour studies according to research design are not always easy. Next, the theoretical orientations of the contributors to this volume are examined; these are frankly diverse and probably irreconcilable. Therefore the final question, concerning 'a unified science of development' raised by Rheingold is apposite. The implied answer appears optimistic; the author hopes that cautious attempts at generalizations about the development of behaviour will continue to be made, and thus give point to further comparative studies. This is the reviewer's hope, too.

W. SLUCKIN

A Textbook of Physiological Psychology. By SEBASTIAN P. GROSSMAN. New York and London: Wiley. Pp. xix + 932. 113*s*.

A book of this size and with this title clearly aims to be a comprehensive survey of the field of physiological psychology. Whether or not one considers it to be comprehensive depends largely on one's definition of physiological psychology. Grossman, a member of the biopsychology department at the University of Chicago, following what, with few exceptions, appears to be standard American practice in writing physiological psychology text-books, emphasizes research with animals at the expense of research with human beings. Thus, comparatively little space is allotted to work of the kind appearing in the journal *Psychophysiology*; the work of Lacey, for example, is given scant attention and the considerable literature on sleep is more or less ignored; sleep–wakefulness is subsumed in the index under the headings, 'alerting response', 'arousal responses', 'habituation' and 'orienting responses'. This is a pity, since the last text-book on physiological psychology to appear, the third edition of Morgan's *Physiological Psychology* suffered from much the same faults, and although it did include a chapter concerned with sleep, arousal and activity much of the material was somewhat out of date. Other omissions in Grossman's book include the role of the brain in the mediation of language functions together with the evidence on, and the theoretical implications of, the effects of brain damage on 'intellectual' functions in man. Developmental changes in the central nervous system with their behavioural implications are also not dealt with.

However, if one accepts these limitations on the subject-matter of physiological psychology, then Grossman's treatment of what remains is a very good one. Part I (177 pp.) is a detailed coverage of the properties of cells and of nervous transmission, and of the anatomy of the central, peripheral and autonomic nervous system. Biochemical factors are given greater coverage than is usual, which adds to the usefulness of these chapters. Part II (138 pp.). comprises a discussion of one sensory system, namely vision (audition is briefly examined in Part I), a consideration of the motor system, mechanisms of sensory motor integration and of the reticular formation, together with the non-specific thalamic projection system. Part III (404 pp.) is the longest section, accounting for nearly half the book and is concerned with needs and drives, hunger, thirst and sex, fields in which Grossman himself has made important contributions; it continues with a chapter on emotional behaviour, a particularly good survey of the effects of central stimulation and concludes with a chapter on psycho-physiological theories of motivation, in which Grossman makes several useful criticisms of existing theories and adds, albeit tentatively, a new one of his own, basic to which is a distinction between homeostatic and non-homeostatic drive mechanisms. Finally, Part IV (258 pp.), in this reviewer's opinion the best section of the book, is devoted to learning and memory, with chapters on electrophysiological correlates of learning, the effects of central nervous system stimulation and lesions on acquisition and retention, consolidation, physiological theories of learning and biochemical hypotheses. In this section an extremely complex research area is clearly expounded, and the many factors involved are treated in considerable detail, both individually, and as they relate to each other. However, here again human studies are seldom mentioned, with the exception of a few studies concerned with habituation and the orienting response. As far as the treatment of memory is concerned, a discussion of the work by Penfield, Milner and their colleagues on memory changes following temporal lobe removal and hippocampal damage would perhaps, have been relevant, although this research does receive a very brief mention in the chapter on emotional behaviour in Part III.

This book makes few concessions to the reader and the style is condensed. Each point made is extremely well documented. This makes the book useful for research workers, since they can gain an over-all view of a particular field, together with a discussion of related research areas as well as acquiring relatively painlessly, a large number of up-to-date references to the research literature. The book also contains an unusually large number of diagrams and figures, the majority of which are reproduced from the research literature.

The book as a whole is undoubtedly very good value for money, containing more pages and more information than many conference proceedings which often have only a limited appeal and are generally more highly priced. Apart from its value for research workers it would certainly be useful to undergraduates in the third year of their honours courses in psychology. The sophistication it assumes on the part of the reader may well be daunting for most first-year and even second-year students. However, for students intending to specialize in physiological psychology this book will be a source of information and ideas for some time to come.

D. R. DAVIES

The Predicament of the Family: a Psycho-Analytical Symposium. Edited by PETER LOMAS. London: Hogarth. 1967. Pp. 219. 36*s*.

Lest the title mislead, it should be said that this collection of essays is about the influence of family interaction upon individual members rather than any strain the family may be undergoing as a social institution in contemporary society. As a whole, the volume illustrates a number of changes in psychoanalytic interest and activity. The contributors are concerned mainly with real rather than phantasied family relationships and they tend to prefer observation of current family behaviour to retrospective reconstruction in analysis as a means of gathering data. Analysis is regarded as a particularly valuable source of ideas about family functioning and there is support for this contention in the essays, although only Laing, Bowlby and Rhona

Rapoport attempt to conceptualize in systematic form. The contributions by Bowlby on childhood mourning (a revision of his 1961 Adolf Meyer lecture) and Rapoport on courtship and marriage are especially notable also for their empirical testing of psychoanalytically derived hypotheses and their integration of this research within the general disciplines of psychology and sociology. These two papers represent the culmination of the developments illustrated in the book and demonstrate the potentialities of a non-exclusive psychoanalytic approach.

DEREK JEHU

Creativity: Its Educational Implications. Edited by J. C. GOWAN, G. D. DEMOS, E. P. TORRANCE. New York and London: Wiley. 1967. Pp. xii + 336. 64*s*.

This is a collection of thirty-six reprinted papers by a number of writers on topics such as 'Must creative development be left to chance?', 'Creativity and the curriculum', 'The counselor and the creative child' and 'Suggestions for mothering the gifted to encourage curiosity, learning and creativity'. The contributors range from psychologists very well known for work on creativity, such as Guilford and MacKinnon, through numerous Professors of Education (many of them much less well known in this country) to one 'housewife' whose contribution is by no means the weakest.

The book is competently edited and put together and the papers have been sensibly grouped in sections, some of which will primarily interest the psychologist, some the parent, and so on. Nevertheless, the over-all impression is that this is just one more in the interminable procession of symposia on creativity, particularly as it appears to contain very few facts or ideas that are not already familiar from similar collections.

The main justification for its publication could have been the need to inform teachers of recent discoveries by psychologists; this is probably the main aim but is inadequately fulfilled. Some contributors, for instance, carefully explain or define what they mean by 'creativity'; others use the term as a kind of pat on the head for almost any praiseworthy characteristic. Nor is sufficient care often taken to make clear what is speculation, and what is well supported by evidence and generally accepted. For instance, in one section written by two of the editors, it is stated categorically that 'creativity is a gift from what Erikson (1950) calls the narcissistic or imitative period of life which occupies the child from age four to about seven.'

A variety of readers will find something of interest in this book, but few will find it particularly useful as a work of reference.

H. J. BUTCHER

Personality Measurement: An Introduction. By BENJAMIN KLEINMUNTZ. Homewood, Ill.: Dorsey Press. 1967. Pp. xiii + 463. $7.95, college price.

The aim of the author was to assemble his teaching materials about personality tests and testing in one place, and from this it is clear that his lecture coverage was comprehensive and thorough. The books ends fashionably in a discussion of the use of computers to simulate personality, and of physiological and sensorimotor methods of measurement. The 800 or so references cover the period up to 1966 in the more accessible journals.

The book is planned in four parts. The first two are general: an introduction, and personality testing approaches. The others are specific in reference: personality tests and current investigatory approaches. The classification in Part 3 could perhaps be improved by adding a category relating to the measurement of personality by means of profiles of cognitive proclivities and skills. This would obviate the necessity for defining the Rorschach in the same terms as projective tests of the TAT type (i.e. as indicating wishes, needs and desires) and would suggest the inclusion of Wechsler profiles.

There is a good summary and reading list at the end of each chapter, and if the book is judged on actual content it may be regarded as a useful text in association with amplifying lectures. As an introductory book, however, the level of insight and background demanded is uneven. University teachers in this field would find it a useful reference book to possess.

M. E. HEBRON

Timing in Temporal Tracking. By J. A. MICHON. Soesterberg, The Netherlands: National Defence Research Organization TNO. 1967. Pp. vi+127. (Price not given.)

This Ph.D. thesis describes the study of a novel task: how well people can predict a series of clicks when the interval between clicks varies like the input in tracking. The subject usually pressed a morse key, and had to try and make his responses synchronize with the clicks. A number of different inputs were presented such as ramps, steps and sine waves. There was also an input which depended upon the man's response: the clicks occurred at a fixed time interval after the man's response, instead of after the previous click. In each case the data of each individual were fitted by a model similar to the models used for the human operator by control system engineers. To a reviewer interested in tracking and bored by time perception, these experiments come like a much-needed breath of fresh air. It is a pity that Michon is not more familiar with the tracking literature. There are similarities between his experiments and the quite numerous experiments on intermittent display presentation, of which he appears to be unaware.

The last three experiments introduce an additional mental load. In an integrated condition the subject had to make multiple-choice responses in time with the clicks, according to which one of two, four or eight lamps came on immediately he made the last response. In a dual-task condition the multiple-choice responses had to be made with the left hand while the subject responded to the clicks on the morse key with his right hand. The integrated condition had little effect upon the mean error, but when time was short, variability increased with the number of choices. The dual task produced still more variability in that the man sometimes failed to respond to a step for a click or two. The experimental results are rather spoilt by the use of the same six experimental subjects throughout, which can produce unknown transfer effects. However, Michon is to be congratulated upon his ingeniously new approach to an old problem.

CHRISTOPHER POULTON

Philosophy and the Science of Behaviour. By MERLE B. TURNER. New York: Appleton-Century-Crofts. 1967. Pp. xvii+539. $9.50.

Dr Turner takes as her subject the examination of 'certain aspects of behaviouristic psychology in the context of the philosophy of science'. This delineation cuts out a number of the most volubly debated areas in psychology. For instance, the problems surrounding psychoanalysis are hardly mentioned; nor is the development and adaptation of existentialism. Yet this leaves a large number of problems to be dealt with, and *Philosophy and the Science of Behaviour* is a very ambitious book, in which both the history of psychological thought and its present complex and confusing state are reviewed as a whole, perhaps for the first time. There are certainly few books which attempt to give so comprehensive a picture of the problems and philosophical underpinnings of psychology. The first part, entitled 'Backgrounds in Empiricism', traces the development of the varying traditions of thought which still determine the questions we ask. This is followed by a discussion of psychology in the context of the contemporary philosophy of science: the language of psychology, the proper functions of theories and models, the nature of explanation, and the various aspects of reductionism are all dealt with in this section. The final section of the book, 'Probability and Inference', covers the theory and use of statistics, including Bayesian statistics.

This is an awe-inspiring table of contents, seeming, at first glance, to indicate a book which should be compulsory reading for every psychologist. But closer acquaintance is disappointing. Dr Turner certainly covers the ground she sets out to deal with, and covers it in great detail—each section is followed by notes intended to provide chapter and verse for various points made more briefly in the text itself. Yet I found it increasingly difficult to decide which audience Dr Turner had in mind as she wrote. I had originally thought that the book would be of interest to three groups: fully fledged psychologists, philosophers with a particular interest in the philosophy of mind, and undergraduate students in psychology. The undergraduates were the first of my mental audiences to leave, baffled by the degree of philosophical expertise necessary for making any headway at all. In fact, Dr Turner's approach demands an acquaintance with philosophical language and methods which many psychologists will find a stumbling-block. If she intended a book primarily for philosophers she has done a useful job in rounding up facts and arguments which are usually only found widely scattered amongst the journals. If she was writing for psychologists, a less involved style, and a non-technical vocabulary would have been better. As it is, her matter is for psychologists; but her manner is not.

ALISON WEBB

The Anatomy of Achievement Motivation. By H. HECKHAUSEN. Translated by K. F. BUTLER, R. C. BIRNEY and D. C. MCCLELLAND. New York and London: Academic Press. 1967. Pp. xvii+215. 48*s*.

In the *Annual Review of Psychology* for 1962, Jenness warned would-be students of the achievement motive that their initiation ceremony included the reading of three thick books. This thin volume might appear to make only a small additional demand, but its slimness misleads. The coverage extends to 650 references, many to recent American work, but with some Japanese and British contributions accruing to the reading list. More daunting are the 50 references to untranslated post-war German studies, of which Heckhausen's own earlier book is the most important.

Heckhausen achieves this extensive international coverage with a brevity that is bought at a cost. The review is written from the author's own theoretical perspective and this presumes a set sufficiently different from current American thinking about the achievement of motive to render reading difficult. He stresses the motivating relevance of cognitive rather than affective factors, and emphasizes the fluctuating quality of motivation with situational variations. He discusses the interaction of 'motive to avoid failure' and 'motive to achieve' without a commitment to theories of anxiety based upon reinforcement principles. Other suggestions are equally challenging, but within the space available Heckhausen whets rather than satisfies the appetite. The reader needs more information to think within the author's framework and to evaluate its possible advantages.

This regret fades in the second half of the book, particularly during the four chapters surrounding performance in an achievement-related situation. Here the text follows a temporal sequence: general orientation to performance, specific setting of goals, performance itself, and subsequent reactions and evaluations. These chapters have a continuing theme, while the others deal with such separate topics as the reliability, the validity, and the correlates of nAch scores.

This book should be read by all who like to keep abreast with developments in the achievement motive field. Those with an interest in the measurement of the motive to avoid failure, or the development of children's experiences of success and failure, will be encouraged to follow up some untranslated reports.

W. P. ROBINSON

Foundations of Social Psychology. By EDWARD E. JONES and HAROLD B. GERARD. London: Wiley. 1967. Pp. vii+743. 68*s*.

This is a textbook for psychologists. The customary homage to sociological insights is not paid by two authors devoted to the experimental approach.

The book has many virtues. It attempts, not too rigidly, to link the two major areas of socialization and social interaction with the common thread of man's dependence on his fellows both for knowledge ('information dependence') and for rewards ('effect dependence'). From this perspective, cognitive development is given generous treatment, despite a perverse neglect of Piaget and other sources of 'non-experimental' data. The approach is honest, tenacious, but humourless; complex issues are never shirked for the sake of a superficial simplicity or a racy style, and many questions are suggested that as yet cannot be answered. Systematic in approach (a fact for which the authors apologize), the coverage is nonetheless good. No major area of experimental inquiry is omitted, and many fields new to social psychology textbooks are included—the experimental work on curiosity is just one instance.

Although this is not an easy book to read, the authors strive hard to forge meaningful links between chapters. The trouble is not, as it were, between chapters, but within. Frequently, researches are paraded in full dress in a remorseless procession; the significance of each is easily lost on the reader, who must make haste to the excellent chapter summaries for enlightenment. The balance between documentation of research on the one hand and discussion and orientation on the other would be more appropriate to a handbook than a textbook.

The varied and incomparable contribution of Leon Festinger to social psychology is made evident, and Kurt Lewin is celebrated as the father of experimental social psychology. It is in the earlier chapters on child socialization that some of the most telling questions emerge. For example, the authors spell out the theoretical implications of the child's 'information dependence', the practical consequences of which are largely unknown. The need for more observation in the field could not have been better revealed.

Teachers of social psychology will make good use of this book, for it demands a reappraisal of familiar topics, and develops valuable, neglected concepts—'cue control' for example, as opposed to 'outcome control' as a technique of social influence. It fails as an introductory textbook because it is too much a documentary, and because the authors take for granted too high a level of interest in the strategy and tactics of research. In its contribution to the development of the subject, it compares well with other recent textbooks. Less of a catalogue than Secord and Backman, more comprehensive than Newcomb and with a systematic orientation lacking in Brown, and Jones and Gerard, it enables us to take stock of the experimental enterprise in social psychology. As an advanced course textbook, as a handbook for the research worker and as a goad for the teacher it will have considerable influence.

GEOFFREY M. STEPHENSON

Opportunities and Limitations in the Treatment of Alcoholics. Compiled and edited by Joseph Hirsch. Springfield, Ill.: Thomas. 1967. Pp. xi+103. $5.75.

This is a collection of papers given at a symposium on alcoholism in the United States, mainly for practising physicians. It summarizes a good deal of information about alcoholism and its treatment, but, as might be expected with such an audience, concentrates on medical more than on psychological aspects. It is also expository in character, containing little that would be new to the expert in the field.

Psychological interest is concentrated in one chapter, entitled 'Psychotherapy with the Alcoholic', by Dr Peter Hartocollis from the Menninger Foundation. This describes various psychotherapeutic approaches to psychological treatment and briefly considers the disadvantages of some of them, though it does not attempt any thorough evaluation. It is nevertheless useful as an indication of the kind of work done, ranging from psychoanalysis to counselling or aversion methods. Group therapy is particularly favoured with alcoholics. There is also some discussion of compulsory treatment, and of whether therapy should be enforced when the alcoholic is still drinking.

Some of our more ardent behaviour therapists ought to ponder the unanimous view of the contributors to this book that aversion treatment without attention to the social and personal problems of the alcoholic is no solution. On the other hand, the conception of alcoholism as a chronic disease, calling for regular treatment possibly throughout life, strikes as shrewdly at the psychotherapist, with his belief in excessive drinking as a 'symptom', as it does at the behaviour therapist.

HOWARD JONES

Personality and Arousal: A Psychophysiological Study of Psychiatric Disorder. By Gordon S. Claridge. Oxford: Pergamon Press. 1967. Pp. xviii+274. 70*s*.

In recent years there has been growing interest in the possibility that individual differences in 'arousability' may underlie some of the more important dimensions of personality. Dr Claridge's book, an important contribution to this debate, contains an account of a series of experimental studies of the behaviour of dysthymic, hysterico-psychopathic and psychotic soldiers, together with some new speculations arising out of his results.

Claridge takes his starting-point in Eysenck's work on the physiological basis of extraversion and neuroticism. This work has sometimes suffered in the past from receiving either no criticism at all or criticism of a purely destructive kind. The heartening thing about Claridge's book is that it contains constructive criticism, based on both empirical data and theoretical argument. The points he stresses as requiring a modification in the Eysenckian scheme are, first, the high likelihood that extraversion and neuroticism are not functionally independent dimensions; second, that the differences between dysthymics and hysterics cannot be accounted for solely in terms of differences in degree of extraversion; thirdly, that many of the phenomena accounted for by Eysenck in terms of differences in the activity of some sort of inhibitory process can equally well be accounted for in terms of some sort of excitatory process; and finally, that, if one identifies this excitatory process with 'arousal', there are equally good grounds for an arousal interpretation of either neuroticism or extraversion.

Claridge's own major contribution undoubtedly lies in the data he reports. There are many individual experimental results which deserve attention. The Shagass sedation threshold emerges as an extremely valuable tool, Claridge's results in general paralleling those of Shagass. (There is an excellent final chapter by Herrington on the pharmacology of the barbiturates which will be of value not only to investigators using the sedation threshold but also to many others.) Perhaps Claridge's most important findings relate to the factor-analytic pattern emerging from a battery of presumed tests of arousability. These suggest the existence of two separate arousal-type processes; and, to the extent that they function together (as revealed by a single second-order factor), they are related to both extraversion and neuroticism. The experimental data on psychotics are also of great interest: the intriguing change of sign of correlation between measures of arousal (previously noted by Venables) appears again.

On the basis of his data, Claridge attempts to construct an alternative theoretical framework in which to incorporate both psychotic and neurotic disorders. This attempt is based on the notion that there are two systems regulating level of arousal: a 'tonic arousal system' and an 'arousal modulating system'. He argues that differences in their level of activity and in their modes of interaction can account for the observed findings, both in his own experiments and over a much wider field. His theory is provocative, and should supply more grist for the research mill. One or two comments might be made before the wheels start turning.

In the first place, most of the empirical findings invoked as support for his theory were derived from a sample of military neurotics and controls which was, as he points out, badly biased in its E and N scores on the MPI. This makes interpretation a little difficult: some of the studies in which dysthymics are differentiated from hysterics are contradictory to previous findings; in some cases, hysterics do not differ from controls; and in others neurotics do not differ from controls. Because of the sampling bias, it is difficult to maintain a systematic interpretation of the results. Claridge tends to ignore these discrepancies, preferring to concentrate on correlates of the barbiturate sedation threshold, which he argues is the best index of 'arousability'. But this may be begging the issue, especially when he is intent on showing that an 'arousal' theory is a better basis for an attack on the causation of psychiatric disorder than an Eysenckian 'inhibition' theory.

Secondly, there is an unfortunate looseness of fit between the theoretical constructs introduced by Claridge and the methods he suggests by which to measure them. He shows that he is himself aware of this by, for example, his uneasy comment (p. 191) on the use of the spiral after-effect as a measure of the level of activity in the arousal modulating system.

This said, however, we hope that *Personality and Arousal* will be widely read.

JEFFREY A. GRAY
WILLIAM ORWIN

Br. J. Psychol. (1968), 59, 2, *pp.* 193–194
Printed in Great Britain

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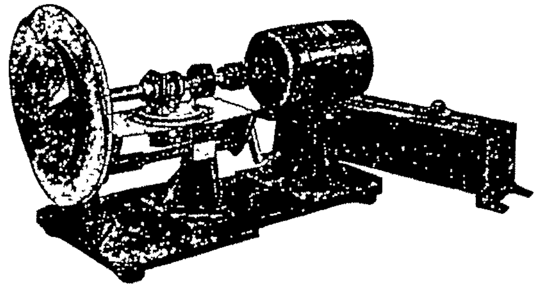

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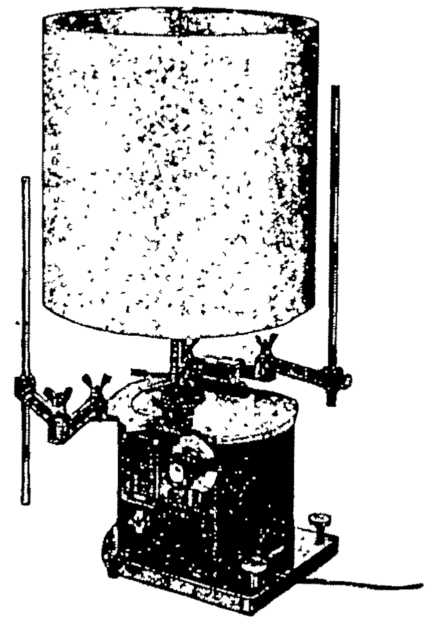

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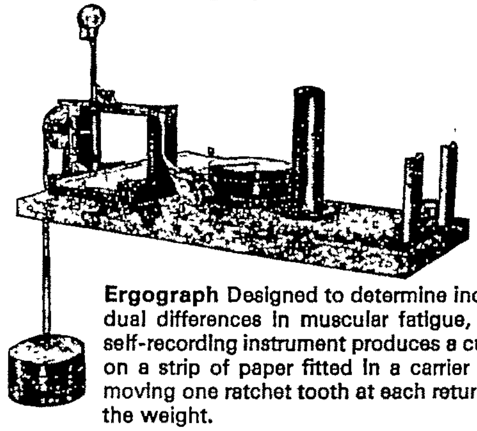

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Das Buch vereinigt in einer recht einzigartigen Weise die Darstellung der vorliegenden empirisch-wissenschaftlichen Untersuchungsbefunde mit der konkreten Darstellung der praktisch-psychoterapeutischen Tätigkeit.

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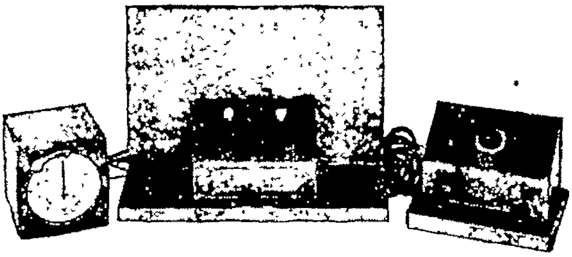

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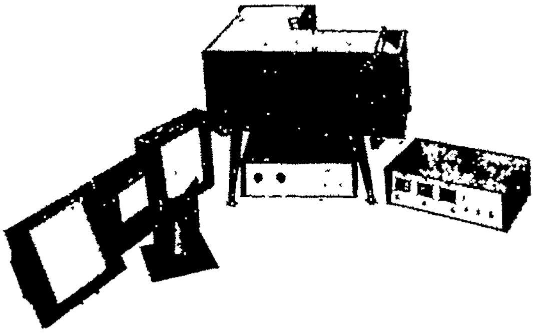

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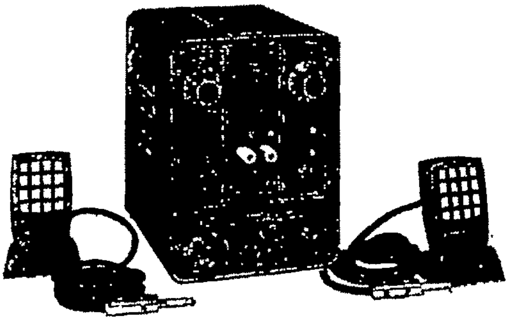

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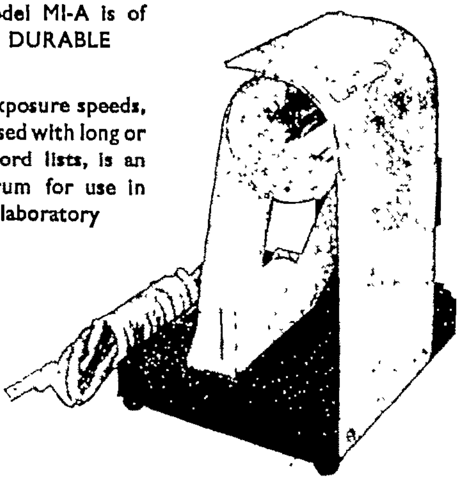

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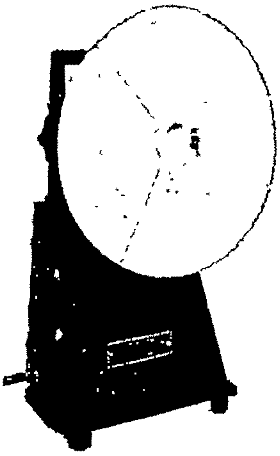

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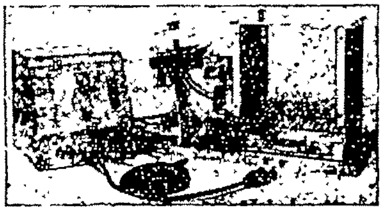

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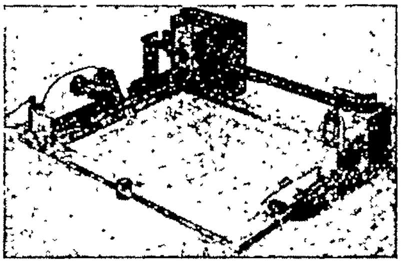

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DIFFICULTIES IN PICTORIAL DEPTH PERCEPTION IN AFRICA

By JAN B. DEREGOWSKI

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Hudson's Pictorial Perception test and a construction test, in which subjects were asked to construct geometrical models shown on pictures, were administered to Central African schoolboys and domestic servants. It was found that a significant proportion of subjects who were judged to be two-dimensional perceivers on Hudson's test built three-dimensional models; the validity of Littlejohn's and Du Toit's explanation is therefore questioned. The models thus obtained were, however, often both distorted and oddly oriented. Domestic servants were found to be more often two-dimensional perceivers than schoolboys; it appears therefore that passive exposure to pictorial material can play only a minor role in determining pictorial depth perception. It is suggested, on the basis of the results obtained, that the two-dimensional responses on Hudson's test may be due, at least in part, to the subjects' inability to organize the material presented in the test pictures.

Cultural differences in pictorial representation and their relations to psychological factors have been investigated by Thouless (1933), who concluded that the 'greater tendency to phenomenal regression to the real object' which was found in Indian students when compared with British students might be responsible for some conventions of Indian art. Nadel (1937) confirmed the existence of a relation between art and perception by comparing responses to pictorial material of two neighbouring West African tribes who inhabit a similar environment but differ in their culture. Hudson (1960, 1962*a*, *b*) took the problem a step further by preparing a special test and by postulating that a specific difficulty, namely that of perception of depth in pictures, was present in the case of Negro Africans. This hypothesis led to extensive experimentation and subsequently to the conclusion that the difficulties which Negroes experience in pictorial depth perception were due to cultural and possibly to genetic factors, the most important of the former being the lack of exposure to pictorial material. The existence of such difficulties was confirmed by Winter (1963), who asked Negro subjects to interpret safety posters. On the other hand, Littlejohn (1963), on the basis of his study of the Temne concept of space, suggests that this concept, which differs markedly from the Western concept, might be responsible for the difficulty; and Du Toit (1966) that the difficulty may be due to the structure of Bantu languages.

The following experiment was intended to investigate whether the difficulties described occur equally frequently with all types of drawings as the above theories imply and whether a prolonged exposure to an urban westernized environment leads to a significant increase in the frequency of 'westernized' responses.

It was decided to use, in addition to a standard depth perception test (Hudson's test), a test involving more abstract and probably more ambiguous stimuli on which one would be inclined to postulate on the basis of the published data a more frequent depth perception failure. Domestic men-servants were thought to constitute a population stratum of relatively low educational level which has been exposed to a westernized environment and which had access to magazines and came into daily contact with pictorial material in various other forms. This stratum was therefore sampled. A sample of schoolboys who apart from school-hours had probably less contact with pictorial material was also used.

METHOD

Experimental tasks

1. *Hudson's Pictorial Depth Perception Test*

(*a*) *Materials.* This test is issued in booklet form and consists of six drawings and a photograph of a three-dimensional model. The figures are those reproduced in Hudson's (1960) paper and referred to as: P. 1, P. 2, P. 3, P. 4, P. 5, P. 6, and figure 3. In this experiment the order of presentation was as given above with the sole exception that, following the instructions in the booklet, P. 4 preceded P. 3. Hudson (1967) describes the test as follows: 'Outline drawings and a photograph of a model scene were constructed so as to depend perceptually on the cues of object size, overlap and perspective. The three objects, hunter, antelope and elephant, were drawn in a constant relationship to one another in all outline drawings. The first two objects, hunter and antelope, were drawn so as to be perceived as lying in the same frontal plane with the elephant in the background. Supplementary depth cues were introduced in the form of a hill on which the elephant was standing' (P. 1, P. 2, and P. 3), 'contour lines representing mountains' (P. 2 and P. 3), 'and perspective lines representing a road, vanishing in a horizon' (P. 4, P. 5, and P. 6). 'In all pictures the hunter's spear was aligned on both elephant and antelope.' Also, in all pictures the elephant is drawn on the plane of the paper directly between the man and the buck.

(*b*) *Procedure.* The booklet was held in a clamp in the vertical plane 18 in. from the edge of the table nearest to the subject, and at about the subject's eye-level. The subject was allowed to move freely in the chair in which he was seated and to lean forward if he so desired. One picture at a time was displayed by the experimenter turning the pages. The questions were asked from a questionnaire by a research assistant proficient in local languages, who also recorded the answers. In administering the test the instructions contained in the booklet were adhered to with a single modification, namely the subjects were asked to identify all the subjects shown in the first three pictures instead of all the objects in the first picture and only selected objects on pictures P. 2 and P. 4. The modification was introduced to simplify the procedure. All the following questions, with the exception of question 1, were put to the subjects as they viewed each of the pictures in turn; question 1 was only asked in relation to the first three pictures of the test (P. 1, P. 2 and P. 4).

Q. 1: What do you see? (The subjects were asked to identify each object in the first, second and third pictures, i.e. P. 1, P. 2 and P. 4. If they omitted any items, these items were pointed out and the subjects asked to identify them.)

Q. 2: What is the man doing? (If the answer did not indicate at whom the man was throwing his spear, the subject was asked: 'What is the man doing with his spear?')

Q. 3: Can the buck see the man?

Q. 4: How do you know?

Q. 5: Can the man see the buck?

Q. 6: How do you know?

Q. 7: Which is nearer the man, the elephant or the buck?

Both questions 2 and 7 yield responses which can be classified as indicative of presence or absence of depth perception. In answer to question 2 a response that the hunter is killing a buck was taken to indicate 3D perception and that he is killing an elephant to be indicative of 2D perception. Similarly an answer 'buck' to question 7 classified the response as 3D and an answer 'elephant' classified it as 2D.

2. *Construction tasks*

Materials. The following materials were provided for all tasks involving construction: 20 plasticine balls about ½ in. dia.; bamboo splints varying in length by ½ in. steps from 2 in. to 7 in., and forming 10 sets of 16 splints each, the splints within each set being equally long.

Preliminary construction tasks

These tasks were introduced to accustom the subjects to the use of the construction materials provided.

(*a*) *Models*. Three models were exhibited on the table in front of the subjects throughout the experiment: (i) vertical—a 6 in. long bamboo splint placed vertically with a plasticine ball at each end; (ii) triangle—three plasticine balls connected by three 5½ in. long bamboo splints forming a triangle, placed flat on the table; (iii) tetrahedron—a similarly constructed tetrahedron, with 5 in. edges.

(*b*) *Procedure*. In each case a model was pointed out to the subject and the subject requested to build a 'thing' like it using wood and clay (plasticine), which were also pointed out to him. Some assistance was given if the subject found it difficult to place the 'vertical' correctly. In such circumstances the plasticine ball in contact with the table was squashed to make it adhere better and to demonstrate to the subject how to make such a model stand.

When the subject had indicated that he had completed his model it was taken to pieces and the subject was given his next task.

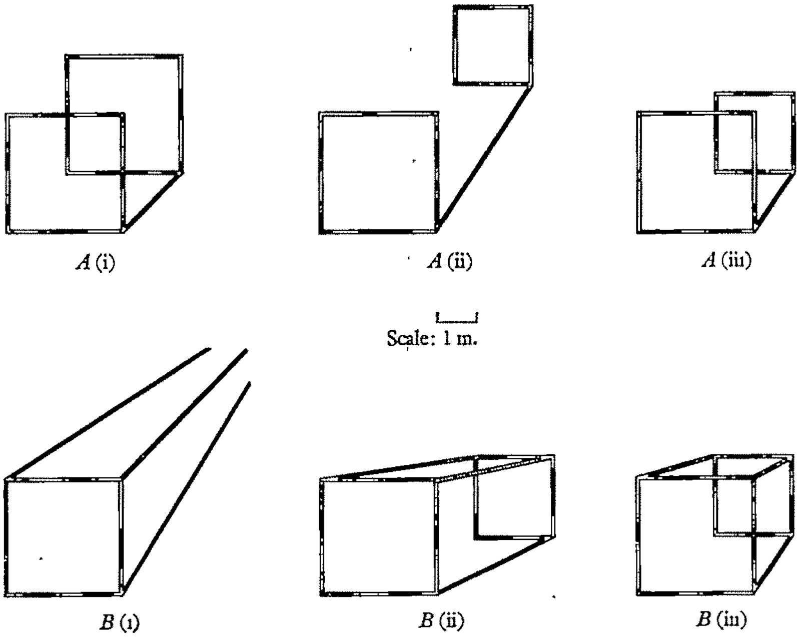

Fig 1. Construction test stimuli.

Construction test

(*a*) *Materials*. The construction materials described above were used, with two sets of drawings (Fig. 1*A*, *B*) which were intended to embody the following depth cues: *A* (i), overlap; *A* (ii), size difference; *A* (iii), size difference and overlap; *B* (i), perspective; *B* (ii), size difference, perspective and 'weak' overlap; *B* (iii), size difference, perspective and 'strong' overlap.

(*b*) *Procedure*. The drawings were presented in a clamp in the same manner as Hudson's test booklet. Each subject was instructed: 'I want you now to build what you see on the picture.' Any further inquiries were met with: 'Just as you see on the picture.' When the subject indicated that he had completed his model the experimenter judged this model to be '2D' (two-dimensional) or '3D' (three-dimensional), recorded his judgement and made a sketch of the model. The model was then taken to pieces, and the next drawing presented with the same instructions.

The criteria for 3D and 2D judgements were as follows: a response was judged to be 3D if all the members did not lie in parallel or nearly parallel planes separated by a distance not exceeding ½ in.; all the other responses were classified as being 2D. No difficulty was experienced in applying this criterion as no models fell within the borderline region.

Only one of the two sets of drawings was used with each subject, the presentation order within the set being varied in a systematic manner to ensure that each sequence was experienced with about equal frequency in each group of subjects.

Table 1. *Characteristics of the subjects*

	Schoolboys			Men-servants		
	Mean	S.D.	Range	Mean	S.D.	Range
Chronological age (yr.)	11·9	2·6	7–16	30·3	9·2	16–58
Years of schooling	3·9	1·8	0–7	3·8	2·3	0–8
Years of domestic service	—	—	—	9·3	9·8	0–34

Table 2. *Arrangement of experimental treatments*

Group code numbers and no. of subjects (in parentheses)	Hudson's test	Construction of			Construction test	
		Vertical	Triangle	Tetrahedron	Set A	Set B
1*a* (6) and 5*a* (6)	1	2	3	—	4	—
1*b* (6) and 5*b* (6)	4	1	2	—	3	—
2*a* (6) and 6*a* (7)	1	2	—	3	4	—
2*b* (6) and 6*b* (5)	4	1	—	2	3	—
3*a* (6) and 7*a* (6)	1	2	3	—	—	4
3*b* (6) and 7*b* (6)	4	1	2	—	—	3
4*a* (7) and 8*a* (6)	1	2	—	3	—	4
4*b* (5) and 8*b* (6)	4	1	—	2	—	3

Subjects

The subjects came from various Bantu tribes and had been living in the town of Lusaka for a considerable time; samples were drawn from two populations: (1) 'schoolboys', all of whom attended the local primary school and were sons of Bantu African drivers, messengers and estate labourers of various establishments lying on the extreme edge of Lusaka; (2) 'men-servants' employed by private individuals and living in various parts of Lusaka. Characteristics of these samples are given in Table 1.

Experimental plan

At the beginning of each experimental run each subject was told that he was at liberty to withdraw if he wished to; this was necessary to allay anxiety, especially in the case of adult subjects. The subject's personal data were recorded: his age, number of academic years of schooling and, in the case of domestic servants, number of years spent in such service. The subject was then allocated at random to one of the treatment arrangements shown in Table 2.

RESULTS

Of the total of 138 subjects tested, 11 were rejected because they failed to identify one of the animals on Hudson's test; such a failure removes the familiar size cue present in all seven pictures and makes interpretation of the responses difficult. A further seven subjects were rejected for various other reasons, such as not attempting to respond or procedural errors, leaving 60 subjects in each sample who completed both tests. From each sample 48 subjects were then selected to form four groups matched approximately for age. The eight groups thus obtained form balanced samples, and further analysis has been confined to the data obtained from them.

Preliminary analysis

The number of 2D and 3D responses on both Hudson's test and the construction test were compared using Fisher's exact probability test to check on the possibility of pooling experimental data derived from the groups which did the same tasks, but in a different order (subgroups *a* and *b*). As the differences were found to be small ($P > 0{\cdot}09$) it was concluded that pooling was permissible.

Differences between subjects in groups from the same sample, who were put through different treatments (i.e. groups 1, 2, 3 and 4 in the case of schoolboys, and groups 5, 6, 7 and 8 in the case of men-servants) were examined by carrying out analysis of variance on the following variables: chronological age, years of schooling, and in the case of the men-servants, number of years spent in domestic service. Logarithmic transformations were used in the case of the first and the last variables, the distributions of which were skewed. The F-ratios obtained were less than unity except for years of schooling of the men-servants ($0{\cdot}1 > P > 0{\cdot}05$). It is therefore legitimate to consider the groups compared as equivalent in all respects, except perhaps for education in the case of the men-servants; but since educational experience in their case was remote in time, and since the differences were small in any case, it was concluded that these groups could reasonably be considered equivalent also.

Analysis of results on Hudson's test

Examination of Hudson's test data revealed that question 7 evoked a 3D response more often than question 1 for all pictures where there was a difference in the frequency of 3D responses, with the exception of P.1; in this case, however, the difference in frequency was found to be non-significant ($P > 0{\cdot}05$) by McNemar's test for the significance of changes (Siegel, 1956). Hence only responses to question 7 have been considered in further analysis.

Table 3. *Distribution of 2D and 3D perceivers on Hudson's and the construction tests*

	Hudson's test							
	Schoolboys							
Drawings ...	Set A				Set B			
Group number ...	1		2		3		4	
	3D	2D	3D	2D	3D	2D	3D	2D
Construction test								
3D	2	2	0	6	0	8	3	8
2D	0	8	0	6	1	3	0	1
	Domestic servants							
	5		6		7		8	
Construction test								
3D	0	3	0	9	0	5	0	8
2D	0	9	0	3	0	7	0	4

Any subject who gave 3D responses to all seven pictures is termed a 3D perceiver and any subject who failed to do so a 2D perceiver. Analogously, any subject who gave three 3D responses to the construction test is termed a 3D perceiver and any subject who failed to do so a 2D perceiver.

The resulting classification of subjects is given in Table 3.

Fisher's exact probability tests (or χ^2 tests where frequencies permitted) yielded non-significant results for comparisons of responders to Hudson's pictures of any two groups of subjects within each main sample of boys or of men (in all cases $P > 0{\cdot}17$). As far as Hudson's test is concerned, it was therefore permissible to group responses within each of the two main sample groups.

A further Fisher's exact test was carried out to see if it would be permissible to group together the two sample groups. Such a grouping was not found to be permissible, the schoolboys being three-dimensional perceivers significantly more often than the men-servants ($P < 0{\cdot}02$).

Comparison between construction test results

Fisher's exact tests were similarly carried out to determine which of the groups could be pooled as far as the construction test results are concerned. It was found that the only groupings permissible were: (i) for schoolboys, group 1 with group 2, and group 3 with group 4; (ii) for domestic servants, group 5 with group 7, and group 6 with group 8. The differences between these groups did not approach $P = 0{\cdot}05$. Thus it appears that the set derived from the introductory task determined the domestic servants' responses, but did not influence the responses of the school-children.

Comparison of performance of the same subjects on Hudson's and construction tests

The probabilities of getting the differences obtained were calculated using the binomial test in place of McNemar's test as the 'expected' frequencies were small (Siegel, 1956). The data were grouped as follows.

Schoolboys	*Domestic servants*
Groups 1 and 2	Groups 5 and 7
Groups 3 and 4	Groups 6 and 8

In all cases $P < 0{\cdot}02$.

Therefore subjects who gave different responses to the two tests respond three-dimensionally significantly more often to the construction test.

Comparison of performance of Hudson's (1960) *experimental groups and groups used in this experiment on Hudson's test*

As no equivalent groups exist, these comparisons are made with what appear to be the most closely approximate groups. Thus the group of school-children used here is compared with Hudson's group 'h' ('school-children, black, standard 6, aged 14–20') and the domestic servants are compared with Hudson's group 'b' ('mine labourers, black, primary education'). For the purpose of this comparison the procedure adopted was that used by Hudson; the pictures were grouped in accordance with the depth cues they provide and χ^2 was calculated for each cue. Only those subjects who consistently, within each cue, perceive pictures three-dimensionally have been included and the data obtained with the help of the photograph have been excluded. The data are given in Table 4.

Within the size cue grouping Hudson's schoolboys were significantly ($P < 0{\cdot}01$) more often three-dimensional perceivers. There was no significant difference between the two groups on other cues ($P > 0{\cdot}05$).

The corresponding comparison for the domestic servant group yields $P > 0{\cdot}05$ on all available data (Table 4). Comparison of responses to the photograph can be made only for the schoolboy sample, for the relevant data for the servants are absent from Hudson's paper. Hudson's schoolboys were more frequently ($P < 0{\cdot}01$) three-dimensional perceivers than the corresponding subjects used in this experiment.

Table 4. *Frequencies of responses to various depth cues; Hudson's data and the data obtained in this experiment*

	Schoolboys					Domestic servants				
	Hudson's data		Data obtained in this expt.			Hudson's data		Data obtained in this expt.		
Depth cue	3D	2D	3D	2D	P	3D	2D	3D	2D	P
Size (P. 1)	17	17	8	40	$< 0{\cdot}01$	0	54	0	48	$> 0{\cdot}05$
Size and overlap (P. 2, P. 3)	19	15	17	31	$> 0{\cdot}05$	—	—	3	45	—
Size and perspective (P. 4, P. 5, P. 6)	12	22	16	32	$> 0{\cdot}05$	0	54	2	46	$> 0{\cdot}05$
Photograph	26	8	19	29	$< 0{\cdot}01$	—	—	—	—	—

Table 5. *Hudson's test responses and corresponding identification frequencies obtained with figure P.* 2

	Hudson's test responses by:			
	Schoolboys		Domestic servants	
	3D	2D	3D	2D
Correct identification of all items on figure P. 2	21	22	4	26
Incorrect identification of some of the items on figure P. 2	3	2	1	17

Recognition of objects represented in Hudson's test

Analysis of identification of objects shown in picture P. 2 was carried out. This picture was chosen because the depth cues it contained were more numerous than those of P. 1. Fisher's exact probability test was used to determine whether identification and three-dimensional perception were related. No such relation was found ($P > 0{\cdot}05$).

The data used for this analysis are given in Table 5.

A similar analysis of responses to P. 4 was hindered by their heterogeneity. There is no generally known term for horizon in the vernaculars and the line representing the horizon was described in a great variety of terms.

'Correctness' of 3D models obtained in the construction test

A model was defined as correct if it was a 3D interpretation of the drawing, having the correct number of members and bearing evidence that essential features of the drawing had been reproduced. (Squares are reproduced as squares or rectangles and are in parallel planes. There is the correct number of members at every junction.) All responses have been evaluated in accordance with this criterion. The evaluations are recorded in Table 6.

Table 6. *Ratios of 'correct' to 'incorrect' 3D responses on construction test*

(The figures give absolute numbers.)

Experimental group	Drawing					
	A (i)	*A* (ii)	*A* (iii)	*B* (i)	*B* (ii)	*B* (iii)
Schoolboys						
Groups 1 and 2	8:3	4:8	6:7	—	—	—
Groups 3 and 4	—	—	—	5:14	8:12	9:13
Domestic servants						
Group 5	3:0	3:0	2:1	—	—	—
Group 6	6:3	8:2	6:4	—	—	—
Group 7	—	—	—	2:4	3:3	2:3
Group 8	—	—	—	4:7	5:6	3:6

DISCUSSION

The degree of agreement on Hudson's test between Hudson's (1960) results and the results reported here suggests that the conclusions reached probably do apply to the population sampled by Hudson as well as to the present population.

The following differences emerge from comparison of the two samples used in this experiment.

The domestic servants were significantly more two-dimensional than the schoolboys on Hudson's test, much more swayed by the set created by the introductory construction of a triangle or a tetrahedron, and much less influenced by the nature of the drawing in the construction test. Whether this was a function of experience, of age, or of some other factors is impossible to say. There appeared to be a basic difference of approach between the two groups, the domestic servants being much more careful, accurate and slow. It might be that, in spite of explanation to the contrary, the domestic servants considered themselves to be taking part in an examination (it was indeed necessary during the experiment to dismiss one of these subjects, who could not be convinced that he was not sitting for an examination). A suggestion that education played no role in determining the ability which could be made on the basis of these results is not acceptable as it is impossible to compare the education received by the schoolboys of today with that received by the servants when they attended school and because, as has already been pointed out, the education of the servants is remote in time. Further, such a suggestion runs against the results obtained by Hudson (1960). It is apparent, however, that the daily exposure of the servants to a pictorially rather rich environment did not affect their responses and that they did not differ from mine labourers, whose contact with pictorial material was presumably less extensive. It may well be that rather passive contact devoid of any feedback has very little effect on this aspect of perception, that an improvement in the performance of domestic servants would occur had the pictorial conventions been explained to them, or had the way in which they interpret pictures been made of some significance.

The frequency with which subjects who were 2D on Hudson's test made 3D responses to the construction test suggests that it is probably illegitimate to extrapolate from Hudson's findings to all types of pictorial material. A subject, it appears, cannot be classified as a 2D perceiver of all pictorial material merely because he is a 2D perceiver as far as Hudson's test is concerned. This does not invalidate Hudson's remarks about the difficulties which might arise owing to the cross-cultural differences in pictorial perception (Hudson, 1960, 1962*a*, *b*). It does, however, limit their applicability by excluding, at least in part, the type of pictorial material used in the construction test. Such a modification applies solely to pictorial representation of depth using conventionalized cues, and does not bear upon the ability to reconstruct the picture accurately in the form of a three-dimensional model. As is shown by Table 6, a large proportion of subjects who were judged to be 3D perceivers did in fact construct figures which were inaccurate. No account was taken of these inaccuracies nor of the orientation of the models when classifying them as 2D or 3D. Many of the models were incomplete (e.g. had the lower horizontal sides of the squares missing); some could only be described as 'gothic' in that they contained many superfluous members, sometimes in an extremely strange array. Moreover, some of the constructions contained unexpected distortions, although the number of members used was correct. Thus Fig. 1*A* (i) was sometimes constructed as a horizontal square flat on the table with another square in a vertical plane above it supported by a single stick in a flag and flagpole manner. Further, the orientation of the models constructed often bore no relation to what might perhaps be considered to be an obvious orientation of the drawing. Thus, for example, Fig. 1*A* (iii) was sometimes constructed as a square three-legged table, or as such a table resting upside down.

The relation between ability to recognize individual objects in a picture and the ability to recognize intended spatial relationships remains obscure. Hudson (1960) noted that correct identification does not predicate 3D perception. The results of this experiment confirm this. It seems that recognition of objects represented in a picture assists depth perception but is not sufficient for it. Thus in the case of schoolboys (Table 5) 3D perceivers are more likely to be found among the subjects who identify the objects ($P < 0{\cdot}02$); the subjects who fail to identify the objects are, however, as likely to be 2D perceivers as 3D perceivers. This suggests that the subjects' inability to perceive pictures three-dimensionally lay, at least in part, not in a failure to recognize various objects shown in the pictures, but in a failure to integrate these representations 'correctly'. The inability of the subjects to construct correct 3D models cannot be easily attributed to lack of manual dexterity as the subjects were able to construct correct 3D models when performing introductory construction tasks. Here the three-dimensionality of the drawing is recognized but the relative arrangements of diverse parts of these abstract but cohesive objects are very often distorted. Hence it may as well be that the subjects do not fail to see depth because they fail to recognize the depth cues as such, but rather because, whilst looking at the pictures, they restructure them by allocating lesser importance to depth cues than to other elements. However, if these cues are given sufficient prominence by the draughtsman, as in the construction test, they are taken account of, even though the objects in question were probably novel to the subjects. It is possible, therefore, that the mode of integration is a product of acculturation and is responsible for the large

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ECOLOGY, PERCEPTUAL DEVELOPMENT AND THE MÜLLER-LYER ILLUSION

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Two separate traditions of research into the Müller-Lyer illusion have existed for at least 70 years: the ecological and the developmental. To assess the ecological hypothesis, a sufficient range in visual ecology typically has been sought in cross-cultural comparisons; however, many of these comparisons have been inconclusive, especially when other ethnic variables have been held constant. These insignificant findings are considered to result from a confounding of the ecological and developmental variables. Appropriate sampling from Temne and Eskimo populations (enabling, in turn, one variable to be matched while the other is varied) demonstrates this confounding within cultures and, when eliminated, the significant influence of each variable on susceptibility to the Muller-Lyer illusion. Further work, however, is needed to discover other factors which will account for variance in susceptibility between cultures.

It is generally agreed that any theory of perception must be able to account for illusion and to explain those occasions on which the processing of information results, not in accurate contact with the world, but in systematic distortion. It is not surprising, then, to find that there exist many theoretical attempts to explain illusion, and that, in their haste to find a solution, one theoretical approach typically takes little notice of another.

Two of these theoretical approaches (the 'ecological' and the 'developmental') have been established at least since the beginning of this century, but only recently (Dawson, 1963) have they been considered in the same work. This report intends to examine these two positions and to show how, in some cases, the variables confound one another, yielding insignificant results. However, it will be shown (using Eskimo and African samples) that, when one class of variables is held constant by appropriate sampling, significant differences in line with both hypotheses do result.

ECOLOGICAL DETERMINANTS

Rivers (1901, 1905) compared the susceptibility of Murray islanders, Todas and Englishmen to the Müller-Lyer and horizontal–vertical illusions and found that while the Europeans were less susceptible to the horizontal–vertical illusion, they were more susceptible to the Müller-Lyer illusion than were the two non-European samples. He concluded that these two illusions belonged to different categories and attempted to explain this difference in terms of 'a difference in the direction of attention', resulting from opposing types of experience in widely differing ecologies.

The suggestion that the illusions belonged to different categories was accepted by Herskovits *et al.* (1956) when they produced an illusion booklet to be carried into the field by anthropologists. In presenting their results (Segall *et al.*, 1963, 1966), their interpretation (similar to that of Gregory, 1963) was based upon the existence of two distinct perceptual inference systems, both of which result in the interpretation of two-dimensional information as three-dimensional space:

(1) In a *carpentered* environment, acute and obtuse angles on the retina are normally

a result of right angles in the environment which merely appear acute or obtuse because they occur in three dimensions. When presented with actual two-dimensional angles, a similar inference is made, producing the Müller-Lyer illusion.

(2) In an *open vista* environment, vertical projections on the retina are normally due to horizontal lines extending away from the observer. When presented with an actually vertical line, a similar inference is made, producing the horizontal–vertical illusion.

It is usually assumed that European samples are more highly 'carpentered' than non-European samples, while they have less 'open vista'. Comparisons between these two groups usually (but not invariably) yield significant differences in susceptibility to these two categories of illusion (Heuse, 1957; Morgan, 1959; Bonte, 1962; Mundy-Castle & Nelson, 1962).

These long-range comparisons of European and non-European samples were considered in two studies (Gregor & MacPherson, 1965; Berry, 1966*a*) to be introducing unnecessary variables, and that a more adequate test of the ecological hypothesis should sample from a single ethnic group with two distinct levels of 'carpenteredness' and 'open vista'. However, neither study was able to show a significant difference in susceptibility to the two illusions between the high and low 'carpentered' and open and closed 'vista' samples. Jahoda (1966), as well, was not able to accept the ecological hypothesis when studied within Ghanaian society, and suggested that the complexity of the problem demanded more specific hypotheses.

DEVELOPMENTAL DETERMINANTS

The second research tradition stems at least from Binet's (1895) work which showed that susceptibility to the Müller-Lyer illusion decreases with age, and this was later confirmed by Wapner & Werner (1957) and by Sun (1964). These consistent age trends, however, are not reported for the horizontal–vertical illusion.

Parallel research within the framework of Witkin *et al.* (1962) has demonstrated that *field-dependent* individuals are more susceptible to the Müller-Lyer illusion than are *field-independent* persons (Gardner, 1957, 1961; McGurk, 1965). That is, the more perceptually developed a person is (as measured by Witkin's EFT or Kohs Blocks), the less susceptible he is to this illusion. The horizontal–vertical illusion, however, shows no consistent relation with perceptual development, which is in keeping with the lack of age changes. The following discussion will thus concentrate on the Müller-Lyer illusion, which is relevant to both the developmental and the ecological hypotheses.

The confounding of ecological and developmental determinants

As we have seen, cross-cultural studies of the ecological hypothesis have rarely been unequivocal in its support: where the comparison has been between European and non-European samples, some support has usually been found; but where the comparison has been between subsamples from a single ethnic group (Gregor & McPherson, 1965; Berry, 1966*a*; Jahoda, 1966), no support has been found.

It is considered that these insignificant differences might be a result of a confounding of the hypothesized ecological and developmental determinants: In

European–non-European comparisons, these two factors vary greatly, but the direction of their variation requires *opposite* predictions in susceptibility to the Müller-Lyer illusion. Whereas European samples are typically high in their degree of carpenteredness (predicting ***high*** susceptibility), they are also usually high (Biesheuvel, 1963; Dawson, 1963; Vernon, 1962) in perceptual development (predicting *low* susceptibility). But it is possible that the first hypothesized determinant might be strong enough to override the second and produce occasionally significant results. In comparisons within one ethnic group, the same confounding occurs; in the rural, traditional areas where there is little carpenteredness, there is also usually lower perceptual development (Berry, 1966*a*; Dawson, 1963) than in the urban transitional areas, where education and informal perceptual training (from the cinema, magazines and posters) are available. Once again opposite predictions would be made by the two hypotheses.

In the light of this analysis, it is not surprising that the most recent studies have found no significant differences in susceptibility to the Müller-Lyer illusion. However, this analysis does suggest a method for studying the effects of the two hypothesized determinants:

(1) Select two samples from the same ethnic group which vary in ecology, but are matched on perceptual development. In this case, the more carpentered sample should be the more susceptible.

(2) Select two samples from the same ethnic group which vary in perceptual development, but are matched on ecology. In this case, the less perceptually developed sample should be the more susceptible.

METHOD

Samples. Samples were drawn from two societies: the Eskimo of Baffin Island and the Temne of Sierra Leone. In each society one rural/traditional and one urban/transitional sample was tested, and these two types of sample were matched between societies for degree of western contact, education, occupation, age, sex and visual acuity. Table 1 indicates the name, location, size and ecology of each of the four samples.

Procedure. As part of a larger battery (see Berry, 1966*b*), the illusion booklet of Herskovits *et al.* (1956) was administered to the four samples; standard instructions, distance and angle of regard were used throughout. The illusion data were then cleaned in accordance with the procedure adopted by Segall *et al.* (1963), which tolerated one Guttman error. Kohs Blocks (original series) were also administered to obtain an estimate of perceptual development.

All testing was done in the appropriate native language with the help of an interpreter; adequate lighting was available, and conditions of temperature and seating were conventional for the individual being tested.

Table 1. *Sample size and ecology*

	Urban/transitional			Rural/traditional		
	Area	*n*	Carpentered	Area	*n*	Carpentered
Eskimo (Baffin Island)	Frobisher Bay	31	High	Pond Inlet	91	Moderate
Temne (Sierra Leone)	Port Loko	32	Moderate	Mayola	92	Low

RESULTS

As already noted, there were no significant differences between the subsamples of the two ethnic groups despite differing ecologies and levels of perceptual development.

Table 2 reproduces these results from Berry (1966*a*); the Müller-Lyer score is the mean number of illusion responses, while for Kohs it is the mean score out of a maximum of 131. However, because of two factors, the 3:1 ratio in the number of subjects in the traditional to transitional samples, and sufficient variance in perceptual development in Pond Inlet and of ecology in Mayola, it becomes possible to select from the larger traditional samples those individuals: (1) in Pond Inlet, who match individuals in Frobisher Bay on Kohs Blocks score, while still differing in degree of carpenteredness, and (2) in Mayola, who match the Pt. Loko sample in degree of carpenteredness, while still varying on Kohs Blocks scores. The criteria for making these selections were:

Table 2. *Müller-Lyer and Kohs results for total samples*

	Urban/transitional	Rural/traditional		
Eskimo ...	Frobisher Bay (n = 30)	Pond Inlet (n = 90)		
Ecology ...	High carpentered	Moderate carpentered	t	P
Muller-Lyer				
Mean	4·33	4·18	0·57	n.s.
S.D.	1·19	1·43		
Kohs				
Mean	90·5	78·6	1·89	0·05
S.D.	28·0	30·6		
Temne ...	Port Loko (n = 29)	Mayola (n = 84)		
Ecology ...	Moderate carpentered	Low carpentered		
Muller-Lyer				
Mean	3·08	3·42	1·37	n.s.
S.D.	1·18	1·22		
Kohs				
Mean	15·6	6·4	3·43	0·01
S.D.	14·6	6·9		

(1) *Eskimo.* Thirty subjects were selected from Pond Inlet to match the 30 Frobisher Bay subjects on age, sex and Kohs Blocks score.

(2) *Temne.* Thirty-three subjects were selected from Mayola who lived in the semi-rectangular modification of the traditional round house which was predominant in the village; these modified houses were similar to those predominant in Pt. Loko.

Table 3 shows the results of using these selected samples from the traditional areas to compare with the transitional samples: within Eskimo society a significant difference in susceptibility to the Müller-Lyer illusion emerges as a result of matching Kohs Blocks scores while maintaining the differing degrees of carpenteredness; within Temne society a significant difference in susceptibility to the Müller-Lyer illusion also emerges as a result of matching the samples on carpenteredness while maintaining a similar difference in Kohs Blocks scores.

CONCLUSION

The two research traditions most frequently used in studying the Müller-Lyer illusion are both supported when their mutual confounding is removed by appropriate sampling. It appears that the insignificant results obtained by many investigators may have resulted from this tendency for each determinant to counteract the influence of the other.

Table 3. *Müller-Lyer and Kohs results for selected samples*

	Urban/transitional*	Rural/traditional†		
Eskimo ...	Frobisher Bay ($n = 30$)	Pond Inlet ($n = 30$)		
Ecology ..	High carpentered	Moderate carpentered	t	P
Müller-Lyer				
Mean	4·33	3·77	1·81	0·05
S.D.	1·19	1·20		
Kohs				
Mean	90·5	89·8	—	n.s.
S.D.	28·0	26·7		
Temne ...	Port Loko ($n = 29$)	Mayola ($n = 33$)		
Ecology ...	Moderate carpentered	Moderate carpentered		
Muller-Lyer				
Mean	3·08	3·97	2·86	0·01
S.D.	1·18	1·29		
Kohs				
Mean	15·6	6·6	2·91	0·01
S.D.	14·6	7·0		

* Taken from Table 2. † Selected.

While this conclusion appears to be valid when making intra-cultural comparisons, it is certainly not entirely so for inter-cultural ones. It would be wrong to conclude that the two factors discussed here are sufficient to explain all variation between cultures, for a glance at Table 3 shows that, although Pond Inlet and Mayola (selected) are both moderately carpentered, there is no significant difference in illusion susceptibility despite extreme contrast in Kohs Blocks scores. However, the present analysis seems to have untangled the problem of insignificant intra-cultural susceptibility to the illusion.

A further qualification must also be made, since it was not possible to select for each factor while varying the other in *both* directions within each ethnic group; lack of variation precluded both the matching of carpenteredness between Pond Inlet and Frobisher Bay, and the matching of Kohs Blocks scores between Mayola and Pt. Loko.

Despite these two deficiencies, however, the evidence presented strongly suggests a confounding interaction between the traditional ecological and developmental approaches to the study of the Müller-Lyer illusion. It would be well worth while to attempt further cross-cultural testing in order to identify the unknown factors at present limiting the conclusion to intra-cultural differences and to enable the selection and variation of all factors in both directions.

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ON INTRODUCING SUPRAMINIMAL DATA TO ITEMS WHOSE SOLUTION DEMANDS THE USE OF INDIRECT PROCEDURES

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A study was undertaken to determine the effects of incorporating supraminimal data, i.e. data over and above those logically sufficient for solution, into code items whose solution requires the use of 'indirect' procedures involving the identification of certain coded words via the elimination of all alternative possibilities. Supraminimal data relevant to item solution were found to impede the solution process, but not to a statistically significant degree (at the 0·05 significance level). Supraminimal data not relevant to item solution had a more deleterious effect, being found significantly to impede the solution process at the 0·01 significance level. The study further confirms that problems whose solution demands the use of indirect (eliminative) procedures are intrinsically difficult for undergraduate subjects. It is concluded that when problems are intrinsically difficult for the subjects at hand then the presence of supraminimal data will impede item solution and lead to longer solution times. This will be true for supraminimal data relevant to problem solution, and even more so for supraminimal data not relevant.

It has been demonstrated previously (Campbell, 1965) that undergraduate subjects experience considerable difficulty with code items whose solution demands the use of indirect procedures involving the identification of certain coded words through the elimination of all alternative possibilities. The items used in the previous study, as is usual in studies of human problem-solving, present the subject with just sufficient data for a unique and unequivocal solution. No more data than these are given, nor any fewer. It is, however, possible to introduce to an item data over and above those logically sufficient for solution. These supraminimal data may be either relevant to item solution or not. Supraminimal data relevant to solution will be data that *can* be used in arriving at item solution but their utilization is not necessary. They are superfluous and introduce redundancy. Supraminimal data not relevant to item solution will be data that have no bearing on solution and cannot be used in any way in arriving at one. It might at first be assumed that supraminimal relevant (redundant) data would facilitate problem solution. 'Overcueing', for example, i.e. having additional cues that overdetermine the answer, has been held to aid the acquisition of correct responses in the field of programmed learning (e.g. Skinner, 1963). Supraminimal relevant data are not, however, always an aid. Bricker (1955), for example, has found that when such data are incorporated into redundant stimulus patterns they have a deleterious effect on pattern identification. With regard to supraminimal data not relevant to problem solution, such data generally hinder the solution process. Yet if the irrelevant data are so unlike the relevant data as to be 'obviously' irrelevant they may not hinder solution at all. Thus Duncker (1945), Maier (1930, 1931, 1933), Székeley (1950) and Saugstad (1955) have experimented with 'practical' problems where certain objects relevant to solution are to be selected from a number not relevant. The irrelevant objects obscure the relevant ones only when the two sets of objects are physically or functionally similar. If dissimilar, the irrelevant objects become conspicuous. They are then easily screened out and have little effect. It being apparent that the effect of supraminimal data on problem solution varies according to the situation, the aim of the present study was to determine the effects of introducing such data to the type of code item dealt with in the previous study. The item type lends itself admirably to experimental investigation since it provides a type of problem situation whose constituent elements can be precisely specified and experimentally manipulated and controlled. It was hoped that a study of these items might enable the formulation of general rules concerning the conditions under which supraminimal data either facilitate or impede problem solution, or have no effect.

A DESCRIPTION OF THE ITEM TYPE

Each item consists of five 5-letter words with coded versions of the words, in a different order, alongside. The coded versions of the words are in skeleton form, but sufficient coded letters are given for an unequivocal solution. The order in which the coded words are listed relative to the uncoded words is decided for each item separately by reference to a table of random numbers. The task for the subject is to locate the uncoded counterpart of each coded word. Take, for example, item (i):

DEATH	- *M* - - -	______
ANGER	*Z* - - - *K*	______
FAIRY	O *I* - - -	______
CHURN	- - - - -	______
YEARS	- O - - *X*	______

Item (i): Just sufficient data for solution.

Unlike most code items, which are based on 'rational' coding principles following rules of alphabetical sequence, the coding principles underlying the present items may be described as 'arbitrary'. Item solution depends on structural features of the various sets of words and the positioning of the coded letters. Included in each coded array is a letter pair (not italicized) with which the subject is instructed to begin. Thus in item (i) the starting-point is the pair of O's. These occur in first and second positions, in the third and fifth coded words respectively. These two words therefore represent uncoded words, one of which has a certain letter in first position while the other has this same letter second. Scrutiny reveals that O must stand for A, with the third coded word representing ANGER and the fifth FAIRY.

Since O *I* - - - represents ANGER and - O - - *X* represents FAIRY, *I* must stand for *N* and *X* for *Y*. What the coded letters *Z*, *M* and *K* stand for is not known. Apart, however, from *Z*, *M* and *K* being positive instances of classes of letters unknown they are also negative instances of the classes of letters *A*, *N* and *Y* (the coded equivalents of *A*, *N* and *Y* being already known). Hence the fact that the second coded word has a *Z* in first position and a *K* in fifth position may be interpreted to mean that the second coded word represents an uncoded word that neither begins nor ends with *A*, *N* or *Y*. This eliminates YEARS and CHURN and the second coded word must therefore be DEATH. Since *Z* - - - *K* represents DEATH, *K* stands for *H*. If the first coded word were CHURN it would accordingly have a *K* in second position. This is not so, so the first coded word must be YEARS. The fourth (blank) coded word must then be CHURN, it being the only word left.

In item (i) just sufficient coded letters are given for a unique and unequivocal solution. If the solution is to be attained in a valid manner all the letters must be used, although not necessarily in the sequence outlined above. In item (ii), by way of contrast, there are coded letters (supraminimal data) over and above those sufficient for solution:

BENCH	- *M* - - *J*	______
TOPAZ	*U* - - - *F*	______
CHIRP	X *Q* - - *K*	______
SAINT	- - *D* - -	______
APPLE	- X - *G Y*	______

Item (ii): Supraminimal data relevant to item solution.

All the coded letters in item (ii) have a bearing on solution and all of them can be used. To use them all is not, however, necessary. The solution is overdetermined and the data are redundant. In item (iii) below there are again supraminimal data (*N*, *J* and *G*), this time *not* relevant to solution. Knowing what *N*, *J* or *G* do or do not stand for does not advance item solution in any way.

DARKY	- *M* - - -	______
YEAST	*Z* - - *N B*	______
APTLY	I *O* - *J* -	______
SHARP	- - - - -	______
TEACH	*G* I - - *X*	______

Item (iii): Supraminimal data not relevant to item solution.

Two further items were constructed analogous to item (i) with just sufficient data for solution. These three items form set I of items. The set I items provide a base-line against which the effects of supraminimal data may be assessed. Item (ii) above plus two further items analogous in structure comprise set II of items. Each of the set II items contains supraminimal data (three additional letters) relevant to item solution. Set III of items consists of item (iii) above plus two further analogous items. The set III items all have supraminimal data (three coded letters) *not* relevant to item solution. Copies of the nine items, plus the three sample items that prefaced the test are available from the author.

METHOD

Subjects. Subjects were 12 final-year B.Sc.(Hons.) students at the University of Edinburgh. There are various indications in the literature (e.g. Dale, 1935; Burt, 1949; Wallace, 1952; John, 1957; Cook *et al.*, 1963) that science students have a characteristically different mode of approach to problem situations than do arts students. Science students are more analytical and more aware of problem structure. It was found in the earlier study (Campbell, 1965) that first-year psychology (arts) students tended not to be able to cope with items demanding the use of indirect procedures (or else not to be willing to make the effort to cope with them). Since the aim of the present study was to determine the effect on such items of supraminimal data, it was important to have subjects who could at least cope with the item type. Final-year B.Sc.(Hons.) students were chosen as the most likely candidates.

Procedure. Each of the 12 subjects was tested individually. Each subject did all three sets of items, but the order in which the sets were encountered was varied according to four 3 × 3 balanced

Latin square arrangements. This permitted control over the row and column variables (order of doing the item sets and individual differences), and made for increased precision where, as here, the rows and columns represent sources of variation likely to be of importance but whose effects it is desired to control rather than to assess. Three sample items prefaced the test. The working of the first two sample items was gone through in detail with the subject. The third was done unaided. The solution of this third sample item was subsequently checked, prior to the administration of the test proper. The second and third sample items contained supraminimal data but no explicit reference was made to this fact. Each item in the test was presented on a separate page and each was separately timed. If a solution were offered that was not correct the subject was asked to revise it. The time for revision was added to the original solution time.

RESULTS

The results were analysed in the manner set out by Cochran & Cox (1957, p. 134 ff.). Mean solution times for each set of items (adjusted for residual effects) were as follows:

Set I items (just sufficient data for solution)	207·2 sec.
Set II items (supraminimal data relevant to item solution)	331·7 sec.
Set III items (supraminimal data not relevant to item solution)	728·9 sec.

Table 1. *Analysis of variance—summary table*

Source of variation	D.F.	S.S.	M.S.	*F*
Subjects	11	280,235·99	25,475·00	
Order (within squares)	8	312,143·76	39,017·97	
Treatments*				
Direct effects (unadjusted)	2	458,197·04		
Residual effects (adjusted)	2	6,755·01	3,377·50	0·10
Residual effects (unadjusted)	2	108,630·88		
Direct effects (adjusted)	2	356,320·36	178,160·18	4·99
Error	12	428,480·25	35,706·69	
Total	35	1,383,935·37		

* Amount and type of data presented, i.e. set I *vs.* set II *vs.* set III of items.

The results of the appropriate analysis of variance are set out in Table 1. The *F* ratio for the direct effects of the treatments (adjusted) is significant at $0{\cdot}05 > P > 0{\cdot}01$. The *F* ratio for the treatment residual effects fails to reach the 0·05 level of significance. For the difference in mean solution times (adjusted for residual effects) between any two particular sets of items to be significant (the 'critical difference') there must be a difference of at least 189·8 sec. (for $P = 0{\cdot}05$, two-tailed) and 267·9 sec. (for $P = 0{\cdot}01$, two-tailed.) The actual differences in mean solution times between the various sets of items are as follows:

Set I *vs.* set II	124·5 sec.
Set II *vs.* set III	397·2 sec. ($P < 0{\cdot}01$)
Set I *vs.* set III	521·7 sec. ($P < 0{\cdot}01$)

From the comparison between the set I and set II items it may be concluded that supraminimal letters relevant to item solution impede the solution process, but not to a statistically significant degree (at the 0·05 level of significance). From the comparison between the set I and set III items it may be concluded that supraminimal letters not relevant to item solution significantly impede the solution process, at the 0·01 level of significance. From the comparison between the set II and set III items, for both of which the absolute amount of data is exactly the same, it is further apparent that the effects of supraminimal data in impeding item solution are not due simply to their being 'additional' but depend also on the type of data they are.

DISCUSSION

The results suggest that supraminimal letters, whether relevant to solution or not, act as complicating detail. Compare, for example, items (i) and (ii). In item (i) the coded array has a pair of O's plus five other coded letters. In item (ii) the coded array has a pair of X's and nine other coded letters. All nine of these letters, while potentially useful, are useful only in the appropriate connexion. Having established, via the pair of X's, that the third coded word is APPLE and the fifth SAINT, it is known that *Y* stands for *T*. But that *Y* stands for *T* is a useful piece of information only in relation to the *U* of the second coded word, i.e. it enables the inference that *U* – – – *F* cannot be a word starting with *T*, which eliminates TOPAZ. The knowledge that *Y* stands for *T* is of no use whatever in relation to the *M*, *J*, *F* or *D*, and the greater the number of such other letters present in the item the more probable it will be that the subject will try to use some piece of information (e.g. that *Y* stands for *T*) in an inappropriate connexion. That supraminimal relevant (redundant) data can act as extra, complicating detail has been noted previously, for example by Wertheimer (1945). One of the problems he used in his work with schoolchildren was that of finding the area of a parallelogram. When various auxiliary lines were introduced, intended as aids, they were sometimes not helpful at all. Wertheimer concludes that unless the auxiliary lines are seen in their proper role they become merely added and puzzling complications. That supraminimal data *not* relevant to solution can act as extra, complicating detail has also been noted previously, and the markedly adverse effect on solution times caused by the introduction of supraminimal letters not relevant to item solution is in line with the notion, outlined at the beginning of the present paper, that irrelevant data might be expected to hinder problem solution to the extent that they are not 'obviously' irrelevant. While a host of problems exists as to by how much and in what way two stimuli must differ before the difference between them can be deemed an 'obvious' difference, it is evident that for the set III items there is no ready basis for distinguishing between the letters that are relevant to item solution and those that are not. To this extent the irrelevant letters are certainly not 'obviously' irrelevant.

None of the items used in the present study was solved particularly quickly. The average solution time for the set I items was 207·2 sec. (about 3½ min.) and the average solution time for the set III items was 728·9 sec. (i.e. close on 12 min.). All 12 subjects did, however, eventually arrive at the correct solution to every item. This may at first seem at variance with the results reported by Campbell (1965), where many subjects seemed truly unable to utilize the indirect procedures appropriate to this type of code item. The discrepancy in results may be due to the previous study having employed group testing, and subjects in a group-testing situation may not be willing to make the considerable effort required for the solving of the items.

In the individual testing situation a desire to please the tester was almost certainly operative and the subjects did not want to make a poor showing. The subjects tested individually were also, as pointed out above, specially chosen as people *likely* to be adept at using indirect procedures and working with negative instances. The subjects did prove able to use such procedures, but they cannot be said to have been adept. They displayed a marked tendency to convert negative statements to positive statements unnecessarily. Take, for example, item (i), where the O's establish the third coded word as ANGER and the fifth as FAIRY. This means that O stands for *A*, *I* for *N*, and *X* for *Y*. While it is not at this stage known what the *M*, *Z* or *K* stand for, they are known *not* to stand for *A*, *N* or *Y*. This eliminates YEARS and CHURN, and the second coded word must therefore be DEATH. Discussion with the subjects reveals that rather than proceeding in this manner, using negative instances, most subjects convert the negative '*Z* does not stand for *A*, *N* or *Y*' to the positive '*Z* therefore stands for *C* or *D*' (since CHURN and DEATH are the only possibilities left). This transformation is not logically necessary, but it seems as though, as Donaldson (1959) has suggested, negatives are in some way 'distrusted'. From comments made by the subjects on completion of the testing it is clear that they found the items a strain, even for those items without the added complication of supraminimal data. The present study accordingly again confirms the suggestion of Bruner *et al.* (1956) that a lack of facility with indirect procedures may extend to a variety of cognitive activities. It may further be concluded that in conditions where the type of problem confronting the subject is difficult *per se*, the presence of supraminimal data relevant to solution and, even more so, of supraminimal data not relevant to solution will hinder the solution process and act as extra, complicating detail.

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THE USE OF SYNTAX IN UNDERSTANDING SENTENCES

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The present study explored the processes by which people understand sentences. Simple sentences with subjects, verbs and objects, but with specific semantic anomalies, were constructed from naturally occurring sentences. Sentences so derived were given to participants in Expts. I and III to judge for sensibleness and to participants in Expts. II and IV to alter so that they made more sense. The results showed the existence of two levels of processing: hierarchical and left-to-right. Participants judging sensibleness depended only on a sentence's deep structure, processing its functional relations according to hierarchical priorities—subject first, predicate second, and subject and predicate in combination last. This was true for both actives and passives. Participants altering sentences, however, depended on both deep and surface structure. They showed an additional left-to-right processing, preferring to change words near the end of both actives and passives. This effect was explained by thematic prominence. It was also found that participants treated actors as a restricted part of the universe of things which can be acted upon.

An English speaker appears to understand a sentence while he is hearing it. This observation, taken naively, suggests that people process a sentence for its semantic information in a strict left-to-right order. Recent linguistic work on generative grammars (Chomsky, 1957, 1965), however, implies quite a different process. People must process a sentence for its deep structure—its underlying functional relations—in order to understand it, and this processing is necessarily hierarchical in character. The present paper describes two experiments which uncover some of the psychological properties of this hierarchical processing, and another two experiments which show that this hierarchical processing is independent of a distinct, but real, left-to-right processing of semantic content.

In the spirit of generative grammars, speakers must, at some point, understand sentences in terms of elementary functional relations. These relations, extracted by speakers from the sentences they hear, are contained in the kernel strings (as yet unformed basic sentences) which underlie the sentences. Consider *The clown amused the children.* This sentence contains the functional relations of: subject of the sentence (*the clown*), predicate of the sentence (*amused the children*), main verb (*amuse*), object of the verb (*children*), and so on (Chomsky, 1965). For the present study we singled out kernel strings with transitive verbs, choosing sentences like *The clown amused the children* and *The umpire was hit by the bat.* The functional relations of interest in such sentences were named, for brevity, subject (*clown, bat*), verb (*amuse, hit*) and object (*children, umpire*).

The main question we asked was: How do people process sentences for understanding in terms of these relations? To answer it, we gave participants of Expt. I partly anomalous active sentences to judge for sensibleness. This confirmed that people do process sentences hierarchically. In Expt. II we asked other students to alter the sentences so that they made more sense. These students, while reflecting the hierarchical structure of Expt. I, also seemed to show a left-to-right process. In Expt. III, therefore, we required other students to judge the sensibleness of both actives and passives. Here it was found that the subject, verb and object were processed hierarchically, whether the sentence was in the active or passive voice. Expt. IV confirmed that, indeed, people also processed sentences in a left-to-right manner, for the subjects, verbs and objects of actives and passives were altered in different ways.

A second question we asked was: How do people represent past experience? Such experience is expressed in every sentence someone speaks. For example, speakers will generally accept some nouns as subjects and reject others. This is a reflexion of how speakers have represented experience—some things can act, but others cannot. Corresponding to subjects, verbs and objects, then, there are the semantic relations of actors, actions and goals of actions. These refer simply to the classes of things people will accept as subjects, verbs and objects. The first two experiments, then, were also designed to discover some of the properties of actors, actions and goals.

In the present experiments we have used mostly semantically anomalous sentences, and yet we wish to generalize to the processing of normal sentences. This is appropriate for two reasons. First, as described below, the parts of the anomalous sentences do come from normal ones, and second, it is only by studying how normal processing is disrupted (here by semantic anomalies) that we are able to infer anything about normal processing. This is the rationale of countless other psychological techniques.

EXPERIMENT I

Method

There are three steps in Expt. I. The first step was to have students compose natural sentences; the second was to construct what we will call derived sentences by systematically altering the natural sentences; and the third was to have raters judge the derived sentences for their sensibleness.

Natural sentences were obtained by Clark (1965) from 15- to 17-yr.-old girls who composed simple active and passive declarative sentences. They had been given, among other frames, the sentence frames, *The — —ed the —* and *The — was —ed by the —*, to fill in with words of their own choice. They were told they could use either regular or irregular past-tense verbs. In this manner 120 subjects composed a total of 960 active and 480 passive natural sentences. It was possible to change all passives into actives and vice versa. Expt. I was concerned only with natural and derived active sentences.

A total of 4096 different derived sentences, all in the active voice, were constructed for raters to judge for sensibleness. They comprised the 16 conditions (called 'derived sentences') shown in Table 1. These conditions break down into five main groups. In the 'no agreement' conditions (derived sentences 1–4), the subject, verb and object of the derived sentences were each chosen from a different natural sentence. In the 'subject–verb agreement' conditions (derived sentences 5–8), the subject and verb came from one natural sentence, but the object from another. The derived sentences of the 'subject–object', 'verb–object', and 'complete agreement' conditions were constructed in similar fashion. The derived sentences within the five groups of conditions, however, were each constructed with different properties. Within the 'no agreement' conditions, for example, derived sentence 4 had as its subject, verb and object, respectively, what was originally an object, a verb and a subject of different natural sentences. In all, half the derived sentences had as subjects what were originally objects of natural sentences, and half had as objects what were originally subjects of natural sentences. These properties are also indicated in Table 1.

The 4096 derived sentences were constructed in an iterative scheme, by use of the derivation matrix shown in columns 3–5 of Table 1. First, 16 natural sentences were sampled at random and numbered 1–16. The subjects, verbs and objects of the natural sentences were then mapped on to the subjects, verbs and objects of 16 derived sentences according to the entries of the derivation matrix. As an example, the subject, verb and object of derived sentence 5 were taken, respectively, from the subject and verb of natural sentence 3 and the object of natural sentence 4. This operation produced 16 derived sentences, one for each derived sentence condition. To produce further derived sentences, the 16 natural sentences were renumbered by adding one to each of their indexing numbers and by giving the last sentence an indexing number of one. The renumbered natural sentences were then mapped on to 16 more derived sentences by means of the same derivation matrix. This procedure, by 15 successive renumberings of the natural sentences, generated 16 different sets of 16 derived sentences. Each of these 256 sentences was derived differently from every other; the subject, verb and object of each natural sentence had been used in every derived sentence condition exactly once. In all, 16 different sets of 16 natural sentences were subjected to this iterative operation to produce the total of 4096 derived sentences (256 per condition).

Table 1. *Actual and corrected mean ratings of* 16 *kinds of derived sentences*

(Italicized relations in the same row were taken from the same natural sentence.)

Derived sentence	Kind of agreement	Subject	Verb	Object	Actual mean	Corrected mean
1	No agreement	Subj_{1}	Verb_{5}	Obj_{3}	4·12	4·06
2		Subj_{2}	Verb_{1}	Subj_{10}	4·04	3·95
3		Obj_{15}	Verb_{13}	Obj_{2}	2·89	2·84
4		Obj_{14}	Verb_{9}	Subj_{11}	3·33	3·35
5	Subject–verb agreement	Subj_{3}	Verb_{3}	Obj_{4}	4·52	4·50
6		Subj_{4}	Verb_{4}	Subj_{12}	4·70	4·66
7		Obj_{11}	Verb_{11}	Obj_{1}	3·03	3·02
8		Obj_{12}	Verb_{12}	Subj_{9}	3·25	3·24
9	Subject–object agreement	Subj_{5}	Verb_{2}	Obj_{5}	4·23	4·20
10		Obj_{13}	Verb_{10}	Subj_{13}	3·31	3·31
11	Verb–object agreement	Subj_{7}	Verb_{6}	Obj_{6}	5·65	5·60
12		Subj_{6}	Verb_{14}	Subj_{14}	4·27	4·29
13		Obj_{10}	Verb_{7}	Obj_{7}	3·67	3·61
14		Obj_{9}	Verb_{15}	Subj_{15}	3·08	3·08
15	Complete agreement	Subj_{8}	Verb_{8}	Obj_{8}	6·78	6·78
16		Obj_{16}	Verb_{16}	Subj_{16}	3·39	3·39

Table 2. *Examples of each of the* 16 *kinds of derived sentences*

1. The cowboy read the pipe.
2. The president lassoed the man.
3. The speech smoked the calf.
4. The pipe lassoed the president.
5. The cowboy lassoed the speech.
6. The president read the man.
7. The pipe smoked the calf.
8. The calf lassoed the president.
9. The president lassoed the speech.
10. The pipe read the man.
11. The president smoked the pipe.
12. The cowboy read the president.
13. The speech lassoed the calf.
14. The calf smoked the man.
15. The cowboy lassoed the calf.
16. The speech read the president.

What these derived sentences are like can be appreciated from the examples of each given in Table 2. These 16 derived sentences, however, were *not* constructed from 16 natural sentences, as the above procedure requires, but from only three—*The cowboy lassoed the calf, The president read the speech,* and *The man smoked the pipe.* This was done, since it is easier to see the relation of the derived sentences to the natural ones in such a simplified example. That is, Table 2 is meant only to give the flavour of the derived sentences, not to provide examples of actual experimental material.

Each derived sentence was printed on an IBM data card by an IBM model 557 interpreter. Sixteen raters were each given 256 derived sentences to rate for sensibleness. In a counterbalanced design, each rater received the 16 output sentences from one renumbering of each of the 16 sets of natural sentences. The 256 sentences were well shuffled for each rater.

The raters were students enrolled in introductory psychology at Carnegie–Mellon University fulfilling a course requirement. They were asked to judge the sensibleness of the sentences on a 1–7 rating scale on which 1 meant 'very nonsensical' and 7 meant 'very sensible'. They wrote their ratings directly on each IBM data card, finishing the task in about ½ hr.

Results

The mean ratings for the 16 derived sentence conditions are shown in Table 1. An analysis of variance showed that the 16 means differed significantly, both with the 16 students as the sampling variable ($F = 62{\cdot}57$; 15, 225 d.f.; $P < 0{\cdot}001$), and with the 16 sets of natural sentences as the sampling variable ($F = 88{\cdot}63$; 15, 225 d.f.; $P < 0{\cdot}001$). (All t tests reported below use as their denominator the more conservative error term of the former F test.)

It seemed necessary to apply a correction to these 16 means. Since the natural sentences were drawn at random from our sample of composed sentences, it was possible to find two or more natural sentences in one set of 16 with, say, the identical subject. This meant that derived sentence 11, with verb–object agreement, was in some cases actually a sentence with complete agreement, and should be considered derived sentence 15. The means were corrected by calculating the probabilities for duplications of actors, verbs and objects (all less than 0·04) and subtracting the appropriate ratings from the actual mean ratings; for example, derived sentence 11, which had verb–object agreement, was corrected by taking out the cases, in the arithmetic, for which there was actually subject–verb–object agreement. The corrected ratings, also shown in Table 1, differ only slightly from the actual mean ratings; the ordering of the conditions remains unchanged.

The first comparisons are to be made between the appropriate means to show which subjects and objects raters found acceptable. Several conditions demonstrated that subject-nouns made at least as good objects as did object-nouns. Derived sentences 1–2, 3–4, 5–6 and 7–8 differed in only one way by pairs. Derived sentences 1, 3, 5 and 7 had object-nouns as objects, and the corresponding derived sentences 2, 4, 6 and 8 had subject-nouns as objects. Sentences with subject-nouns were rated 0·20 scale units better than those with object-nouns, a small but significant difference ($t = 2{\cdot}14$; 225 d.f.; $P < 0{\cdot}05$). But subject-nouns made considerably better subjects than did object-nouns. Derived sentences 1 and 3, 2 and 4, 11 and 13, and 12 and 14 differed only as to whether their subjects contained subject-nouns or object-nouns. Sentences with subject-nouns again made more sense, but by the much larger mean scale difference of 1·26 ($t = 13{\cdot}60$; 225 d.f.; $P < 0{\cdot}001$).

A second set of comparisons was made among the means of derived sentences to show the basis of hierarchical processing of sentences. Derived sentences 1, 5, 9, 11 and 15 were compared, since they vary simply in what kind of agreement they contain. The derived sentences, listed from most to least sensible, were those with complete, verb–object, subject–verb, subject–object and no agreement (all pair differences but the last two successive differences were significant with at least $P < 0{\cdot}05$). The two largest differences, however, were between derived sentences with complete and verb–object agreement and between those with verb–object and subject–verb agreement.

Discussion

We will consider two important results of the preceding experiment. (*a*) The raters, clearly, had different lists of candidates for actors and for goals of actions. (*b*) Raters processed sentences using a set of hierarchical priorities by considering the acceptability of subjects first, the acceptability of predicates as a whole second, and the acceptability of the subject and predicate in combination last.

Actors and goals of actions differed in significant respects. By our definition, actors are things which can take actions—no matter how abstract the action—and goals of actions are things which can be acted upon. The raters distinguished actors and goals, but not into mutually exclusive classes. Actors seem to be more narrowly defined than goals, since subjects could generally replace each other more sensibly than they could replace objects. But most important, actors and goals were asymmetrical: subjects could replace objects easily, but not the reverse. This finding implies that actors are a narrowly defined proper subset of the possible goals. Things that act are but a small portion of those things that are acted upon.

A distinction should be made between possible and preferred subjects and objects. A clever speaker could compose a sentence which has as its subject any English noun he cares to choose. *Man*, as well as *honour*, are possible as subjects. Normally, however, an English speaker will not choose any noun as subject but will restrict himself, more or less, to a limited subset of nouns. As Clark (1965) found, he prefers human or at least animate, nouns as subjects and inanimate or abstract nouns as objects. When he thinks of things that act, he usually thinks of humans and animals, but when he thinks of things that are acted upon, he thinks of inanimate objects and abstract concepts. The present results point out that animate things can also serve as objects in the sense that subject–verb combinations allow the possibility with equal ease. The reverse, however, is not true. Inanimate and abstract nouns may not function as subjects very easily. It is the *possible* actors—things that *can* act—which are but a small portion of the *possible* goals of actions—those things that *can* be acted upon.

Speakers process a sentence for sensibleness in some specifiable manner. According to the present results, they place importance on three interrelated semantic requirements: (*a*) the subject must come from the class of possible subjects; (*b*) the predicate must make sense internally; and (*c*) the subject and predicate must make sense together. Breaking any one of these three requirements in a sentence caused a large drop in its rating of sensibleness. Requirement (*a*) was met in sentences which had subject-nouns, but not object-nouns, in the subject position. Not meeting requirement (*a*) alone caused a drop of 1·26 scale units (t = 13·60; 225 d.f.; $P < 0{\cdot}001$). Requirement (*b*) was met in sentences with verb–object agreement, but not in those without. (Verb–object agreement here assumes that the object position contains an object-noun.) Not meeting requirement (*b*) alone caused a drop of 1·16 scale units (t = 6·22; 225 d.f.; $P < 0{\cdot}001$). Requirement (*c*) was met in sentences with complete agreement, but not in those with just verb–object agreement. In sentences not meeting requirement (*c*) alone, there was a drop of 1·18 scale units (t = 6·33; 225 d.f.; $P < 0{\cdot}001$). These three requirements clearly accounted for the largest differences in ratings among the 16 derived sentence conditions.

There was also evidence that requirement (*a*) must be satisfied first, then (*b*), and then (*c*). Requirement (*a*) was more important than (*b*): derived sentence 13, which satisfied (*b*) but not (*a*), was rated less sensible than derived sentence 1, which satisfied (*a*) but not (*b*) ($t = 2{\cdot}42$; 225 d.f.; $P < 0{\cdot}02$). Furthermore, requirements (*a*) and (*b*) were interdependent. Requirement (*b*), that verb and object agree, became more important when requirement (*a*) was satisfied than when it was not—a difference of 0·77 scale units ($t = 2{\cdot}91$; 225 d.f.; $P < 0{\cdot}01$). These last two results both indicate that requirement (*a*) takes precedence over requirement (*b*). Requirement (*c*), that subject and predicate make sense together, must necessarily be last. Sentences with subject–verb and subject–object agreement were only slightly more sensible than sentences with no agreement. Only when the subject and predicate both satisfied requirements (*a*) and (*b*), respectively, was there any substantial increase in sensibleness with agreement between the subject and parts of the predicate. It should be emphasized that this hierarchical ordering describes a set of conditional judgements, e.g. the judgement of the sensibleness of the subject and predicate together is not possible until both the subject and predicate make sense by themselves. There is no evidence that the ordering describes the temporal sequence of these judgements.

The model just presented for the way speakers process simple sentences, for sensibleness at least, predicts that speakers will find fault with partly anomalous sentences in specific ways. The next experiment was designed to test these predictions.

EXPERIMENT II

Method

The 4096 derived sentences of Expt. I were given to 16 other students, also drawn from introductory psychology at Carnegie–Mellon University, in exactly the same form that they were given to the 16 students of Expt. I. Expt. II participants were asked to read each sentence, cross out one lexical word (the subject-noun, verb or object-noun)—any one they chose—and replace it with a new word so that the sentence would make better sense. They were asked to do this for every sentence, even for those which already made good sense.

Results and discussion

Table 3 lists the percentage of subjects, verbs, and objects crossed out and replaced in each of the 16 kinds of derived sentences. The percentages for each derived sentence condition (each row) are based on 256 different sentences, 16 given to each speaker.

The changes students made in the sentences indicate they processed sentences at two independent levels: left-to-right and hierarchically. Again, the results show the difference English speakers perceive between actors and goals of actions.

What appears to be left-to-right processing is most obvious in the sensible sentences, but is evident in other sentences as well. Derived sentence 15 was an unchanged natural sentence and was rated in Expt. I as very sensible. In it students much preferred to change the object, crossing out the subject and verb much less and about equally often. Their preferences were similar for derived sentence 1, the sentences with no internal agreement. The assumption of this argument is that the right-most item—here, the object—is altered most often because it comes nearer the end of the sentence, and not because it is the object. This assumption is tested in Expt. IV.

There was also evidence for hierarchical priorities in processing, like those in

Expt. I, of the subject first, the predicate second, and the two together last. When subjects were of the wrong kind, i.e. object-nouns instead of subject-nouns, speakers changed them with a mean increased frequency of 21 percentage points. Students next demanded a whole predicate that made sense. In derived sentences 5 and 6, with subject–verb agreement, students ignored subject and verb and changed the objects 78 per cent of the time. But when they did not change the object, they changed the verb more often than the subject, presumably to form agreement with the object, resulting in a better predicate. In derived sentence 9, which had subject–object agreement, again students largely ignored the subject and concentrated on changing the verb or object to form a sensible predicate. When predicates were well-formed, they were left relatively undisturbed. In derived sentence 11, with verb–object agreement, many students changed the subject, even though it was of the right class of nouns. When there was verb–object agreement but the subject was of the wrong class, as in derived sentence 13, students strongly objected to the subjects, changing them 71 per cent of the time. The last step in hierarchical processing is to demand that the subject and predicate make sense together. This step follows from evidence which implies that speakers first demand proper subjects and proper predicates independently.

Table 3. *Percentages of subjects, verbs and objects changed for derived sentences to make sense*

(Derived sentence labels correspond to those in Table 1; percentages by rows add up to 100 except for rounding errors.)

Derived sentence	Kind of agreement	Subject	Verb	Object
1	None	19	25	56
2		17	30	54
3		39	23	38
4		41	31	29
5	Subject–verb	8	15	78
6		8	15	78
7		42	18	40
8		39	21	41
9	Subject–object	12	43	45
10		38	34	28
11	Verb–object	47	13	40
12		25	21	53
13		71	12	17
14		42	32	27
15	Complete	24	20	55
16		39	32	29

The asymmetry of subjects and objects was also evident in Expt. II. Object-nouns were changed more often than subject-nouns when used as subjects (a mean difference of 21 percentage points) and when used as objects (3 percentage points). This result closely parallels the difference in Expt. I ratings which indicated that raters preferred subject-nouns in both subject and object positions.

Expt. II found speakers apparently processing sentences left-to-right at one level but hierarchically at another. This hierarchical ordering, however, is partly correlated with a left-to-right direction of processing. To show the independence of these two levels, we designed Expts. III and IV, noting that in actives and passives the ordering of subject, verb, and object is reversed. Expt. III was meant to test whether active and passive forms of the same kernel string are processed equivalently and hence whether the hierarchical model holds under various transformations which preserve sense but not order.

EXPERIMENT III

Method

As in Expt. I, natural sentences were used to construct derived sentences which were given to raters to judge for sensibleness. In all there were 4000 derived sentences, comprising 20 derived sentence conditions. These 20 conditions, in turn, could be classified into four groups of conditions in a 2 × 2 design: five conditions were made up of active sentences derived from natural actives; five, of active sentences derived from natural passives; five, of passive sentences derived from natural actives; and five, of passive sentences derived from natural passives. The five conditions within each of the four groups consisted of the five agreement possibilities: no, subject–verb, subject–object, verb–object and complete agreement (corresponding exactly to derived sentences 1, 5, 9, 11 and 15 of Expt. I). The terms subject, verb and object, of course, refer to the underlying kernel strings for both actives and passives, so that in passives their left-to-right order is object, verb and subject. In this experiment there were no conditions in which subject-nouns were placed in the object position or object-nouns in the subject position.

An iterative scheme identical to that in Expt. I was also used here to generate derived sentences. Twenty sets of 10 active and 10 passive natural sentences—from the same source as in Expt. I—were put through the appropriate 20 × 3 derivation matrix to produce the 4000 derived sentences. An important property of this total number was that for every active there was a corresponding passive with the identical subject, verb and object. The sentences were given to 20 male undergraduates fulfilling a requirement for introductory psychology at The Johns Hopkins University, 200 to each, in a counterbalanced design as in Expt. I. The printing of the cards, shuffling of the decks, instructions and rating scales were the same as those used in Expt. I.

Table 4. *Mean ratings of active and passive derived sentences constructed from active and passive natural sentences*

Form of natural sentence ...	Active		Passive	
Form of derived sentence ...	Active	Passive	Active	Passive
Kind of agreement				
None	3·22	3·14	2·74	2·82
Subject–verb	3·72	3·53	3·26	3·44
Subject–object	3·42	3·40	3·30	3·26
Verb–object	4·91	4·80	4·33	4·32
Complete	6·57	6·35	6·50	6·39

Results

The mean ratings (shown in Table 4) were affected by only two factors: (*a*) the voice—active or passive—of the natural sentences, and (*b*) the kind of agreement within the sentences. Sentences derived from active natural sentences were rated significantly more sensible than those derived from passive natural sentences ($F = 18{\cdot}70$; 1, 3980 d.f.; $P < 0{\cdot}001$). Active and passive sentences derived from the same kernel string, however, were not rated differently. Kind of agreement affected the mean ratings significantly ($F = 409{\cdot}0$; 4, 3980 d.f.; $P < 0{\cdot}001$), but did not interact with the form of the natural or derived sentences. The order of the mean ratings for the five kinds of agreement replicates the findings of Expt. I.

Discussion

According to the last results, sentences were analysed for sensibleness in the same manner, regardless of whether they were active or passive. These results give no evidence which questions the hierarchical process model presented above, for the five kinds of agreement, had the same effects on ratings of active and passive sentences.

On the other hand, the students composed active and passive sentences that had different properties. As can be seen in Table 4, sentences with verb–object agreement were much more sensible when the natural sentence was active than when it was passive; the same holds true of other sentences, particularly those with no agreement. One interpretation of this is that people compose passives so that the object (the initial noun phrase) and verb do not form a very tight unit. This mimics what they do in composing actives from left to right: the subject and verb do not form a tight unit. Thus the 'information'-bearing part of the sentence is the surface predicate, i.e. the phrase following the surface subject (the initial noun phrase), and this is true for both actives and passives (see Clark, 1965). More generally, the lower sensibleness of the passive is related to Clark's (1965) finding that the passives people compose contain a greater variety of words than the actives they compose; the situations described in passives have less in common with each other than the situations described in actives.

It was conjectured that participants in Expt. II altered the objects so often merely because they were the last word processed. One question still remained unanswered: Would students also process/passives in a left-to-right direction? Expt. IV, then, presented both actives and passives to other students to alter in the same manner as in Expt. II.

EXPERIMENT IV

Method

The 4000 derived sentences of Expt. III were given to a new group of 20 students, exactly as they had been given to participants in Expt. III and with the instructions given to participants in Expt. II. That is, students were to cross out any one of the lexical items (any word except *the* and *by*) in each sentence, regardless of its sensibleness, and replace the word with one that increased the sensibleness of the sentence. The students were Carnegie-Mellon undergraduates, as in Expts. I and II.

Results

The percentages of words altered in each of the 20 conditions are shown in Table 5. Each row is based on 200 sentences, 10 altered by each student.

Participants in Expt. IV, as is clear from Table 5, processed both actives and passives in a left-to-right direction. There were very small differences between sentences derived from natural actives and natural passives, but large differences between derived actives and passives. This is just the opposite of the results in Expt. III. Subjects were altered less in actives than in passives, and objects more. This was consistent for all 10 comparisons (significant at $P < 0.001$ by a sign test). The active–passive comparison is clearest in the complete agreement conditions: the subject, verb and object percentages for actives closely match the object, verb and subject percentages for passives.

Table 5. *Percentages of subjects, verbs and objects changed for active and passive derived sentences to make sense*

		Form of natural sentence					
		Active			Passive		
Form of derived sentence	Kind of agreement	Subject	Verb	Object	Subject	Verb	Object
Active	None	14	46	40	18	40	42
	Subject–verb	8	26	65	7	21	72
	Subject–object	8	64	28	12	66	22
	Verb–object	44	24	32	53	21	26
	Complete	18	36	45	26	29	45
Passive	None	20	53	27	26	44	30
	Subject–verb	11	34	55	18	29	53
	Subject–object	12	72	16	21	65	14
	Verb–object	62	23	15	72	22	6
	Complete	45	33	22	40	39	21

GENERAL DISCUSSION

From these experiments emerges the view that people process the meaning of sentences in two separable ways: hierarchically and left-to-right. To review the evidence: Expts. I and III indicated that in rating sensibleness people judged the acceptability of the subject first in priority, the predicate as a whole second, and the subject–predicate combination last; participants in Expt. II altered sentences in a manner consistent with this process. This was a hierarchical process. In Expts. II and IV, on the other hand, there was an additional bias towards altering words further along in sentences, whether active or passive. This indicated a left-to-right process.

The phenomena described as left-to-right and hierarchical processing are closely related to structural descriptions linguists have made of surface and deep structure (Chomsky, 1965). Surface structure, in effect, indicates the structure of the linear succession of what is spoken, while deep structure indicates the functional relations among the spoken linguistic units. The same functional relations from deep structure can occur in sentences with widely varying left-to-right properties or surface structure. Surface structure must therefore be evaluated, at least in part, before deep structure is known. It is not too surprising that we found both components in the present experiments. It is important to make known, however, where and how surface and deep structure must be taken into account in psychological processes.

Judgements of sensibleness, in the present results, were not affected by surface structure. Sensibleness is a property of deep structure, as it should be. The model we presented for the process of judging is parallel to the set of rules in the base component of generative grammars (Chomsky, 1965). In the present description, the smallest constituents are judged for acceptability before the larger ones are. The judgement, in other words, starts at the bottom of the phrase-marker—a tree with labelled nodes—and moves up to successively higher nodes.

How a sentence is to be altered to become more sensible, however, depends on both surface and deep structure. Altering *The women crumpled the ball* is different from altering *The ball was crumpled by the women*, although judgements of sensibleness (and deep structure) are the same for both. This seems to imply that the alteration of sentences is related to the left-to-right production of sentences. Because the realization of the object is first, instead of last, in passives, it is much less alterable after the sentence has been read. It is possible that this is merely a short-term memory phenomenon, and the last item in an auditory memory (see Neisser, 1967) is more readily retrieved and replaced.

Left-to-right processes, however, have been found in numerous other psycholinguistic experiments (Anderson, 1963; Clark, 1965; Duncker, 1945, p. 24; Johnson, 1967; Johnson-Laird, 1968*a*, *b*; Prentice, 1966; Turner & Rommetveit, 1968). The principal result has been that a linguistic unit takes on thematic prominence when it is first in a sentence. In simple sentences, the surface subject (the active's subject and the passive's object) states the theme of the sentence—what the sentence is about—and the remainder of the sentence states what is to be said about the theme (Halliday, 1967*a*, *b*). Thematic prominence can also be used to explain the left-to-right processing in the present study. The students altering sentences were loath to change the theme of the sentence, for psychologically the theme was a fixed topic and not easily altered, but the students were quite willing to change what was said about the theme. At the same time, thematic prominence has little to do with sensibleness, for subjects, verbs and objects make sense together, regardless of whether the subject or object is the theme. An explanation in terms of theme has intuitive appeal for anyone who has tried to alter nonsensical sentences. There is a strong tendency to feel that the theme is fixed (as in the 'mind's eye') and that it is the rest of the sentence that is nonsensical.

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ODOUR QUALITIES: A GLOSSARY OF USAGE

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The meaning of 72 odour qualities is reported in a Glossary. The information was derived from three main studies. Replies to two questionnaires provide information on frequency of understanding, and the meaning of the qualities understood, expressed in the respondents' own words. The odour stimuli used in the experimental investigations are specified and the rationale for their selection is outlined. In Expt. I, 53 odour stimuli were characterized spontaneously, apart from the aid of a Guidance List. The results are then expressed in terms of the frequency of use of the main qualities. In Expt. II the characterization of 45 odour stimuli was directed by the use of a five-point rating scale with 44 carefully chosen qualities. Data are presented separately for experienced and inexperienced subjects. The experimental results relating to each quality are summarized. Some of the wider implications of these studies are discussed.

This paper is the first experimental report of a multidisciplinary project on odour description and odour classification. The literature has already been reviewed *in extenso* (Harper *et al.*, 1968*a*) and may be consulted for additional background information including discussion of the chemical bases of odour quality. The immediate stimulus to pursue these investigations arose from the role of odour in the flavour of foods. Modern microchemical methods are now able to identify many of the constituents of the flavour complex, often at remarkably low concentration levels (e.g. of the order of 10^{-9} g.). Precise information on what is actually perceived, including the effects of blending, is largely conspicuous by its absence. The Glossary provides primary data which should help to clarify the meaning of descriptive terms in common use, at least in the United Kingdom. Its uniqueness lies in the *experimental* data based upon many subjects or respondents. Until recently most of the available information was based upon the views of individuals or small groups mostly with special expertise. For example, *The Givaudanian* (September 1966)—a perfumery house-journal—provides a short statement on the meaning of such words as warm, dry and heady.

The 'Glossary of Odour Qualities' is only one aspect of these experimental investigations. Other reports include a complementary 'Glossary of Odour Stimuli' (Harper *et al.*, 1968*b*), an examination of the pleasantness or unpleasantness of the stimuli used, studies of the variability of the use of different odour qualities, an analysis of individual and group differences and quantitative methods of classifying both qualities and odour stimuli.

QUESTIONNAIRE STUDIES

An analysis of 300 terms used in describing flavours revealed the following facts: (1) a complete absence of abstract terms; (2) definition primarily in terms of specific substances or process; (3) an emphasis upon undesirable or off-flavours; (4) the necessity for clarification of meaning by reference to the particular context. (From Harper, 1956.)

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A selection of odour qualities from this list of terms was supplemented by a close examination of the literature for words used by perfumers, chemists and food scientists. This provided a final selection of 69 words for the questionnaire studies. Twenty respondents described the meaning of 39 words in the first study; in the second there were 46 respondents and 30 words. The respondents were all adults selected according to availability. The small numbers involved and the absence of many striking differences between the two studies make it unnecessary to specify details about the age, sex, etc. of the respondents. The instructions were simply: 'Here is a list of words which might be used to describe smells. Will you please indicate with a tick which of these really means anything to you. Can you give the meaning of the terms you have ticked in your own words?'

Written responses were made in the absence of odour stimuli. The words used are listed in Table 1. Differences between the two studies were mostly trivial, the main exception being that the term ethere*al* is at the bottom of Table 1 (study I), whereas ether***ish*** is at the top (study II). The word 'ethereal' was used by Amoore (1952, 1962) to designate a 'primary odour'.

Table 1. *Questionnaire studies: percentage of meaningful responses to each word*

(Terms used in both studies are given in bold type; terms of similar meaning are italicized.)

Meaningful (%)	Study I (20 respondents, 39 words)	Study II (46 respondents, 30 words)
91–96	None	**Rancid** – sulphurous – petrol-like – burnt – **fragrant** – earthy – ***etherish*** – aromatic
81–90	Sweet – sickly – ***carbolic*** – mothballs – musty – smoky – minty – sweaty – pepperminty – woody – fishy	Camphor – herbal – paint-like – sour – **meaty** – spicy – ***goaty, animal-like***
71–80	Toasted – **rancid** – like ammonia – pungent – lemony – raw vegetable (garlic/onion) – **fragrant** – foul, putrid, decayed – fruity – medicinal – almond	Sharp – yeasty – honey – **vanilla**
61–70	Hyacinth – ***animal*** – broth-like	Chemical – acid – mealy – ***phenolic*** – balsamic
51–60	Floral – buttery – metallic – **meaty** – **vanilla** – faecal	Like custard
41–50	***Crushed green leaves***	Greasy – musk – ***green***
31–40	Overcooked vegetables, (sweet) – **warm**	Cool – **warm**
21–30	Throaty – like domestic solvent – ***ethereal***	None
11–20	Smooth	None

Table 1 has been divided by a line representing 50 per cent understood. Words falling below this level are obviously inefficient for purposes of general communication, although they may still be useful technical terms, whose meaning may be learned (e.g. musk, warm, cool, green). The questionnaire studies provided the first entries in the Glossary in the form of the proportion of respondents finding each word meaningful followed by a résumé of the meaning originally expressed in the respondents' own words.

SELECTION OF ODOUR STIMULI

The importance of proper sampling in perceptual investigations was first emphasized by Brunswik (1956). There are reputed to be about 17,000 different odorous substances (single chemicals) so that any particular investigation can include only a fraction of these. Selection should be by design rather than by accident, otherwise important stimuli or qualities may be omitted with consequent bias in the results. The number required was limited by confining attention to single chemicals of as high a purity as readily available; in consequence, some specific odour qualities (e.g. a meaty odour) could not be well represented. The full range of pleasantness and unpleasantness, the main odour-contributing structures and substances covering the range from low to high threshold, were all represented. Attention was also given to such practical considerations as availability, stability, toxicity and the maximum number of odour stimuli which could reasonably be used. The final selection (53 odour stimuli) is given in Table 2, including details of specification and concentration. Those few stimuli not used in Expt. II are indicated thus (†). The usual solvent was dinonyl phthalate (DNP), but the few chemicals not sufficiently soluble in this were dissolved in water. Otherwise DNP has excellent solvent properties and is relatively odourless *when purified*. Physicochemical data are available which would allow determination of the vapour concentrations of a number of the stimuli. In fact, we have found that DNP was odourless only to a proportion of the subjects, but there is little evidence to indicate an appreciable contribution from the solvent to the perceived odour qualities. Highly trained perfume chemists must probably be excepted from this statement. The most important point is that the stimuli can be reproduced by other investigators. The stimuli were presented in 50 ml. screw-topped glass bottles, each containing 20 ml. of the dissolved stimulus. The subjects were instructed to bring the neck of the bottle, from which they had just removed the cap, to the nose gradually until they obtained a clear impression, concentrations having been adjusted previously so that most people could readily perceive the main qualities without being overpowered. The subjects were also asked not to exhale into the bottle since this would induce chemical changes in the stimulus. Changes were minimized by storing the samples under nitrogen at + 1° C. when not in use. They were frequently checked and replaced if any noticeable changes had occurred.

EXPERIMENT I

Expt. I was originally intended only to be exploratory. However, the responses were so extensive (over 400 persons participated) and the results so informative that they are reported here. They provide most valuable data on spontaneous odour characterization under relatively uncontrolled conditions, mainly by inexperienced (but science-orientated) subjects, which may be compared with the results of the directed form of characterization in Expt. II. The stimuli were presented in groups of seven, selected so as to preserve a balance within each group in terms of pleasantness

Table 2. *Specification of odour stimuli*

(n' = number of subjects giving clear positive responses;
n = number of subjects in Expt. I smelling each stimulus.)

Odour stimuli (and supplier)	% conc. in dinonyl phthalate (or water)	n'/n (Expt. I)
Acetaldehyde* (British Drug Houses)	0·02 v/v aq	40/46
Ammonia (BDH)	2·0 v/v aq	52/56
Benzaldehyde (BDH)	0·5 v/v	67/70
Benzyl acetate (BDH)	0·5 v/v	45/49
Butyric acid (BDH)	0·4 v/v aq	47/51
γ-Butyrolactone* (BDH)	10·0 v/v	41/45
Camphor (BDH)	1·0 w/v	41/46
Coumarin (BDH)	0·1 w/v	35/42
Cyclohexanone (BDH)	10·0 v/v	51/52
Diacetyl* (BDH)	0·02 v/v	53/60
p-Dichlorobenzene (BDH)	2·0 w/v	42/49
Diethyl sulphide (Eastman)	0·05 v/v	52/54
Dimethyl disulphide (BDH)	0·0003 v/v	44/49
2, 2-Dimethyl propanethiol* (Aldrich)	0·02 v/v	53/60
Ethanol (Burroughs)	50·0 v/v aq	41/50
Ethyl acetate (BDH)	1·0 v/v	47/49
Ethyl benzene (BDH)	10·0 v/v	45/46
Eugenol (BDH)	1·0 v/v	44/45
Exaltolide (Fritzsche Bros.; Expt. II: P. T. Petley)	0·4 w/v	34/46
Furan (BDH)	6·0 v/v	49/60
Geranial† (Proprietary Perfumes Ltd.)	0·3 v/v	49/57
Geraniol† (Proprietary Perfumes Ltd.)	1·0 v/v	30/46
Guaiacol (BDH)	4·0 w/v	98/108
1-Heptanol (BDH)	1·0 v/v	46/50
1-Hexanethiol* (Eastman)	0·05 v/v	42/42
1-Hexanol (BDH)	10·0 v/v	50/57
1-Hexylamine (L. Light & Co.)	0·05 v/v	38/48
n-Hexyl *n*-butyrate (K. & K. Labs. Inc.)	10·0 v/v	39/45
Indole (BDH)	1·0 w/v	43/66
Iodoform (BDH)	0·2 w/v	32/48
β-Ionone (BDH)	1·5 v/v	54/63
l-Menthol (BDH)	2·0 w/v	54/63
Methyl ethyl ketone† (BDH)	2·0 v/v	42/44
Methyl salicylate (BDH)	10·0 v/v	44/46
Musk xylol‡ (Proprietary Perfumes Ltd.)	1·0 w/v	31/69
Naphthalene (BDH)	1·0 w/v	44/45
Nitrobenzene (BDH)	10·0 v/v	57/58
1-Octanol (Hopkins & Williams)	0·5 v/v	54/61
Phenol† (BDH)	1·0 w/v aq	48/49
Phenylacetic acid (BDH)	1·5 w/v	41/49
2-Phenyl ethanol (BDH)	0·5 v/v	65/73
2-Picoline (BDH)	0·05 v/v	48/50
α-Pinene† (L. Light & Co. Ltd.)	2·0 v/v	32/46
1-Propanol† (BDH)	10·0 v/v	45/48
2-Propanol (BDH)	10·0 v/v	42/45
Pyridine (BDH)	0·05 v/v aq	44/49
Safrole (BDH)	5·0 v/v	42/44
Skatole (Kodak)	0·1 w/v	34/43
Trimethylamine* (BDH)	0·005 v/v aq	43/50
Vanillin (BDH)	1·0 w/v	223/236
Vinyl ethyl ketone* (K. & K. Labs. Inc.)	0·25 v/v	57/60
Water† } Control		15/45
Dinonyl phthalate (Bush)		24/63

* Sample checked every four weeks. † Not used in Expt. II. ‡ Solid crystals used in Expt. II.

and unpleasantness. Subjects were asked to describe each odour and rate for pleasantness and unpleasantness. Written instructions were used, but these were usually explained orally, including the function of the guidance list. This consisted of 31 distinctive qualities or groups of qualities representing typically how odours might be characterized. An odour may be familiar without it being easy to recognize or to call to mind the relevant descriptive qualities. This is clear from personal experience, from subjects' comments (e.g. 'Oh my god, where's that list!') and from observations in the literature (e.g. Banister & Zangwill, 1941). The following words were included in the guidance list:

almond-like – acid, sharp, pungent – animal, sweaty, faecal – burnt, smoky – carbolic – cool – earthy, musty – fishy – floral – fragrant – fruity – garlic, onion-like – green, grassy, crushed leaves – herbal – meaty – medicinal – menthol, camphor – minty – mothballs – oily – painty – petrol – putrid – rancid – sour – spicy – sulphurous – sweet – warm – woody – yeasty.

Otherwise characterization was spontaneous and most subjects used only one word. The words used were tabulated and grouped; for example, lemony, fruity and acid drops were grouped together as 'fruity'; rose petals, violets, bluebells and hyacinth were grouped as 'floral'. The figures quoted in the Glossary refer to the number of subjects using each quality, per hundred, giving clear positive responses to that particular stimulus. The number giving clear positive responses (n') and the number of subjects (n) smelling the particular stimulus are given in Table 2. Differences between n' and n are probably due to low sensitivity of some individuals to a stimulus (e.g. musk xylol), to fatigue (e.g. hexylamine), or to complex and diffuse qualities in an odour which lead to some subjects being unable to describe certain odours. We stress that the results are reported only in terms of the number of subjects per hundred giving clear positive responses for the dominant qualities.

EXPERIMENT II

In Expt. II two separate groups of subjects—15 who were experienced in smelling chemicals and 20 who were inexperienced—evaluated each stimulus. The data were collected at the subjects' convenience. Environmental conditions varied since it was necessary to go to about half the subjects, whereas the rest carried out the task in a special testing laboratory. Although not air-conditioned, a slight positive air pressure prevented the background odours of the main laboratory from penetrating into the test room.

The number of stimuli was reduced to 45, eliminating some because of unnecessary duplication, others because they had proved demonstrably unstable and still others because they were not available in sufficient purity. The stimuli were presented mainly in groups of five. The extent to which each of 44 carefully selected qualities was perceived was expressed on a scale from 0 (absent) to 5 (present in extreme degree).

One version of the record form is reproduced in Table 3, the four alternatives being designed to minimize sequential effects while still retaining a form easy to transfer to punched cards for analysis. Perceived strength was rated on a similar scale and with a few exceptions the concentrations proved to have been adjusted as intended. Pleasantness or unpleasantness was also rated on a nine-point scale, although this information is not emphasized in the present report. The form of presentation of the experimental data is explained in a key which precedes the Glossary. Briefly, the quantitative results are presented separately for the experienced and the inexperienced groups. Information relating to those qualities which proved to be important is presented in two different forms, a mean rating and the proportion of times that quality was used, without reference to the magnitude of the mean rating. The general position is then summarized.

Table 3. *Score sheet for odour qualities*

Please score numbers on the scale:	Absent	Slightly	Moderately			Extremely
	0	1	2	3	4	5

P		Q	
Aromatic	0 1 2 3 4 5	Fragrant	0 1 2 3 4 5
Meaty (cooked)	0 1 2 3 4 5	Sweaty	0 1 2 3 4 5
Sickly	0 1 2 3 4 5	Almond-like	0 1 2 3 4 5
Musty, earthy, mouldy	0 1 2 3 4 5	Burnt, smoky	0 1 2 3 4 5
Sharp, pungent, acid	0 1 2 3 4 5	Herbal, green, cut grass, etc.	0 1 2 3 4 5
Camphor-like	0 1 2 3 4 5	Etherish, anaesthetic	0 1 2 3 4 5
Light	0 1 2 3 4 5	Sour, acid, vinegar, etc.	0 1 2 3 4 5
Heavy	0 1 2 3 4 5	Like blood, raw meat	0 1 2 3 4 5
Cool, cooling	0 1 2 3 4 5	Dry, powdery	0 1 2 3 4 5
Warm	0 1 2 3 4 5	Like ammonia	0 1 2 3 4 5
Metallic	0 1 2 3 4 5	Disinfectant, carbolic	0 1 2 3 4 5
R		S	
Fruity (citrus)	0 1 2 3 4 5	Oily, fatty	0 1 2 3 4 5
Fruity (other)	0 1 2 3 4 5	Like mothballs	0 1 2 3 4 5
Putrid, foul, decayed	0 1 2 3 4 5	Like petrol, solvent	0 1 2 3 4 5
Woody, resinous	0 1 2 3 4 5	Cooked vegetables	0 1 2 3 4 5
Musk-like	0 1 2 3 4 5	Sweet	0 1 2 3 4 5
Soapy	0 1 2 3 4 5	Fishy	0 1 2 3 4 5
Garlic, onion	0 1 2 3 4 5	Spicy	0 1 2 3 4 5
Animal	0 1 2 3 4 5	Paint-like	0 1 2 3 4 5
Vanilla-like	0 1 2 3 4 5	Rancid	0 1 2 3 4 5
Faecal (dung-like)	0 1 2 3 4 5	Minty, peppermint	0 1 2 3 4 5
Floral	0 1 2 3 4 5	Sulphurous	0 1 2 3 4 5

DISCUSSION

As Zwaardemaker (1925) stressed, the specification of odour qualities is still very much at the stage of 'object language' comparable with the specification of colour before Newton opened up the possible development of objective measurements. However, even the use of object language seems to involve more than one type of mental process. The data suggest that it is necessary to distinguish between the identity relation, in which a particular quality is precisely defined by the substance, and the use of the same term for purpose of general description. For example, most of the highest mean quality ratings (ranging between 4 and 5) refer to the identity relation illustrated by ammonia, mothballs (naphthalene) and vanilla-like. In some instances the 44 qualities rated failed to provide a satisfactory means of characterizing the stimulus, and the qualities added by way of comment were more highly rated. Eugenol, which is better described as clove-like than in terms of any more general quality, is an excellent example. Reference may also be made at this point to the differences in mental set or perceptual attitude implied by spontaneous as opposed to directed forms of characterization. For example, when spontaneously characterized, methyl salicylate was only rarely referred to as 'cool' (2 per cent in Expt. I). In contrast, in directed characterization, cool is used to the greatest extent with methyl salicylate (E: 2·1, 14/15; I: 1·8, 10/20). This suggests that 'cool' is an odour quality not readily perceived unless attention is drawn to it. Perhaps 'experience' itself creates its own predispositions. Several experienced subjects found the task of rating separately some 44 qualities difficult to carry out and some of the experienced subjects used the smallest number of qualities, generally rated at a low level. Perhaps their very experience gives them more precise conceptions of similarity so that they reject as inappropriate qualities which the less experienced might accept. Certainly, with very few exceptions the inexperienced subjects use more qualities than the experienced in the directed form of characterization. Some of the experienced subjects have also noted that once they have *identified* an odour there is a tendency not to seek further qualities, the correct name—object language again—being fully adequate.

Reference should be made here to the problem of 'mixed standards', a term proposed by Harper (1968) but implicit in the original set of reference standards proposed by Crocker & Henderson (1927) and commented favourably upon by Sagarin (1955, p. 45). With few exceptions a particular stimulus is rarely characterized effectively in terms of a single quality. This comment refers, of course, primarily to the composite picture based upon the views of between 10 and 20 subjects, rather than to characterization by a single subject. A single individual will rarely provide a balanced picture which includes all the qualities found in the composite approach. The limitation of attempting to define major qualities by single substances is well illustrated by the following examples:

> ...Whereas almond is the main quality found in benzaldehyde this is also fragrant and sweet.
> ...Whereas oily is best defined by hexylamine for inexperienced subjects this is even more fishy.
> ...Whereas ammonia is most effectively defined by itself it also heads the list of substances in terms of the sharp, pungent acid group of qualities.

One of the most thorough studies resulting in a set of proposed reference standards was reported by Schutz (1964). Where relevant, these standards are quoted in the Glossary, although for specific concentrations reference must be made to the original. Most of these standards are perceptually 'mixed'; that is, they also represent other important qualities. However, within these limits there is good agreement between the present data and those of Schutz's proposed standards for sweet (vanillin) sulphurous (diethyl sulphide), etherish (1- or 2-propanol) and rancid (butyric acid), but poor agreement for spicy (benzaldehyde), fragrant (methyl salicylate) and metallic (hexanol), where few if any of our subjects noted these qualities spontaneously and only at a very low level when directed. These discrepancies raise the question of international or cross-cultural differences in usage. They are not unexpected, for in a series of cross-cultural studies of food habits the greatest single difficulty encountered was the lack of a satisfactory language of taste, odour and texture (Moore, 1964). The 'Glossary of Odour Qualities' presented here may help in this direction.

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APPENDIX

Odour qualities: a glossary of usage

Example from glossary. The following information explains the data given about each odour quality through a specific example. It is assumed here that information is available in all sections, although this is not always the case.

Quality (*e.g. Almond-like*)

Q (15/20, —). Moderately well understood. Nutty, almond essence, etc.

(Q indicates that the data refer to the questionnaire studies. The two fractions in the brackets give the proportion of respondents in each study finding the words given meaningful. Then follows a résumé of the meaning given by the respondents in their own words.)

		Expt. II	
	Expt. I	E	I
Benzaldehyde	84	3·9 (15/15)	4·3 (20/20)
Nitrobenzene	71	3·1 (13/15)	3·7 (17/20)
Cyclohexanone	24	2·1 (11/15)	1·7 (11/20)

A completely satisfactory definition in terms of benzaldehyde, which was also sweet, fragrant and vanilla-like. Many other qualities used by the inexperienced group.

The figures following each quality under the heading of Expt. I indicate the number of times that quality was used per 100 clear respondents. Thus 84 subjects out of 100 giving clear responses referred to benzaldehyde as being almond-like. The data in Expt. II are given separately for the two groups: E (experienced), I (inexperienced). The first two figures in this section give the mean rating for each particular quality. The fraction in brackets indicates the proportion of each group responding positively without regard to the size of the ratings. The final paragraph summarizes the main conclusions of Expt. II. The extent to which the specified quality can be defined by a particular odour stimulus is noted, together with any extremes in the variance involved in the use of the ratings scales. The reference standards proposed by Schutz (1964) are also indicated.

GLOSSARY OF ODOUR QUALITIES

Acid. See Sharp, acid, pungent and Sour, acid, vinegar.

Q (—, 30/46).

Almond-like

Q (15/20, —). Moderately well understood. Nutty, almond essence, etc.

		Expt. II	
	Expt. I	E	I
Benzaldehyde	84	3·9 (15/15)	4·3 (20/20)
Nitrobenzene	71	3·1 (13/15)	3·7 (17/20)
Cyclohexanone	24	2·1 (11/15)	1·7 (11/20)

A completely satisfactory definition in terms of benzaldehyde, which was also sweet, fragrant and vanilla-like. Many other qualities used by the inexperienced group.

Like ammonia

Q (15/20, —). Moderately well understood. Ammonia itself and related products.

		Expt. II	
	Expt. I	E	I
Ammonia	58	4·6 (15/15)	4·5 (19/20)

Used at very high level with ammonia itself, otherwise only at low levels. Used for more stimuli by the inexperienced group than by the experienced.

Animal

Q (12/20, —). Not well understood. Associated with a range of animals—both clean and unclean—and for this reason sometimes being pleasant and other times unpleasant. Several references made to fur.

		Expt. II	
	Expt. I	E	I
Phenyl acetic acid	17	0·1 (2/15)	1·2 (8/20)
γ-Butyrolactone	2	0·5 (4/15)	1·4 (10/20)
Butyric acid	0	0·5 (5/15)	1·3 (8/20)

Not effectively defined by any single stimulus used.

Aromatic

Q (—, 38/46). Well understood, everyday meaning predominated (e.g. a definite pleasant sweet smell, with references to cedar wood and herbs). Seven references to chemical meaning.

		Expt. II	
	Expt. I	E	I
Eugenol	0	1·3 (8/15)	1·6 (13/20)
Safrole	0	1·3 (9/15)	1·7 (12/20)

Not well defined by any single stimulus included in these studies. Widely used, especially by the inexperienced group. Note etymological association between aromatic and spicy.

Balsamic

Q (—, 29/46). Moderately well understood. Signifies a pleasant, medicinal type of smell, e.g. friar's balsam, Canada balsam. Not appreciably used in our experimental investigations.

Like blood, raw meat

Not used in questionnaire studies.

		Expt. II	
	Expt. I	E	I
Hexylamine	0	0·1 (1/15)	0·6 (5/20)
Pyridine	0	0·0 (0/15)	0·6 (5/20)
Diethylsulphide	0	0·2 (2/15)	0·7 (5/20)
Trimethylamine	0	0·1 (2/15)	0·3 (4/20)

Not adequately defined by *any* stimulus used in these studies, not used more than 2/15 by experienced group and 5/20 by the inexperienced.

Broth-like

Q (13/20, —). Not widely understood, but basically correct when known. Reference to watery extract of meat and bones; some included vegetables. Rarely, if ever, used in our studies with single chemicals.

Burnt

Q (—, 44/46). Very well understood. Two main types of response. Toast and wood (pleasant). Burnt milk, saucepans, etc. (unpleasant).

		Expt. II (burnt, smoky)	
	Expt. I	E	I
Hexane thiol	19 (burnt) 7 (smoky)	1·3 (8/15)	1·2 (7/20)
Guaiacol	10 (burnt)	1·5 (9/15)	1·1 (9/20)

Not really well defined by any single stimulus used in these studies. Schutz (1964): reference standard for Burnt—Guaiacol.

Buttery

Q (10/20, —). Not well understood, but responses include fresh buttery, butter used in cooking, on toast or in toffee.

	Expt. I	
Diacetyl	6	(Not used in Expt. II except as a supplementary comment.)
γ-Butyrolactone	0	

Camphor-like

Q (—, 41/46). Well understood. Frequent confusion made between mothballs and camphor. Some refer to both but two persons distinguished definitely between them.

		Expt. II	
	Expt. I	E	I
Camphor	46	2·7 (13/15)	2·2 (14/20)
Naphthalene	11	1·7 (9/15)	2·4 (13/20)
Menthol	19	1·5 (11/15)	1·2 (12/20)

Camphor is well defined by itself, but shows substantial similarity to naphthalene and menthol for all groups.

Carbolic, disinfectant

Q (18/20, —). Well understood. Soap, disinfectant, etc.

		Expt. II	
	Expt. I	E	I
Iodoform	19	0·7 (6/15)	1·2 (8/20)
Guaiacol	16	2·1 (12/15)	1·8 (13/20)
Methyl salicylate	7	2·0 (12/15)	1·8 (12/20)

Moderately well defined by guaiacol, especially for experienced subjects who showed low variance. Used with equal frequency by both groups.

Chemical

Q (—, 27/46). Moderately understood. Various laboratory, industrial and agricultural odours; artificial, sharp and pungent. Term not used in this form in our investigations. Nearest approximation, chemical solvent.

Chemical solvent. See Petrol-like solvent.

Cool

Q (—, 18/46). Not well understood. Associated with menthol, peppermint, mint. Occasional reference to the role of advertising.

	Expt. I	Expt. II (cool, cooling)	
		E	I
Methyl salicylate	2	2·1 (14/15)	1·8 (10/20)
Camphor	2	1·3 (10/15)	1·0 (7/20)
Menthol	4	1·1 (6/15)	1·2 (9/20)
Ethanol	7	0·6 (5/15)	1·6 (13/20)

Well defined for experienced subjects with very low variance for methyl salicylate. The inexperienced groups used the term 'cool' much more widely but with high variance. This quality is probably nor perceived unless attention is directed to it.

Crushed green leaves. See Herbal, green, cut grass, etc.

Q (12/20, —). Not well understood. Included in a deliberate attempt to clarify the perfumer's term 'green'. References made to mown grass, garden manure and various herbs and leaves of plants after crushing.

Like custard

Q (—, 31/46). Moderately well understood. Vanilla, sweet, sometimes sickly. Also milk and custard itself. Not used in Expt. II.

Decayed. See Foul, putrid, decayed.

Disinfectant. See Carbolic.

(*Like*) *domestic solvent.* See Petrol-like solvent, etc.

Q (6/20, —). Very poorly understood. Deliberately included to suggest a class of odours believed to be characteristic and important. Few relevant associations.

Dry, powdery

Not used in questionnaire studies or Expt. I.

	Expt. I	Expt. II	
		E	I
Musk xylol	0	0·7 (7/15)	1·2 (8/20)
Vanillin	0	0·8 (6/15)	0·4 (5/20)

Not well defined by any stimulus used in these studies.

Earthy

Q (—, 43/46). Very well understood. Soil—especially when moist in association with vegetation.

	Expt. I (Musty, earthy)	Expt. II (Musty, earthy, mouldy)	
		E	I
Skatole	12	1·2 (7/15)	1·0 (7/20)
Indole	9	0·9 (6/15)	1·1 (10/20)
Phenylacetic acid	24	0·2 (3/15)	0·7 (7/20)
Butyric acid	6	0·6 (4/15)	1·2 (9/20)

Generally low usage over all substances. Not well defined by any stimulus used in these studies.

Ethereal

Q (7/20, —). Very poorly understood in this form. (See Etherish.) Otherwise anaesthetic, ether, hospitals, light and airy, headache producing.

Etherish

Q (—, 42/46). Well understood. Detailed responses much as for ethereal (i.e. ether, anaesthetic, etc.) Also delicate, sweet, and even sickly.

		Expt. II (Etherish, anaesthetic)	
	Expt. I	E	I
2-Propanol	21	1·9 (13/15)	2·8 (16/20)
Furan	6	2·2 (12/15)	1·9 (12/20)
1-Propanol	11	Not used	

Best defined at a moderate level by 2-propanol for which the experienced group showed low variance. Widely used by both groups. Schutz (1964): reference standard for Etherish—1-Propanol.

Faecal

Q (12/20, —). Not well understood. Otherwise very offensive, sewer-like, excreta in various forms, also babies' napkins.

		Expt. II (Faecal, like dung)	
	Expt. I	E	I
Butyric acid	15	0·2 (1/15)	1·6 (9/20)
γ-Butyrolactone	2	0·3 (3/15)	1·2 (8/20)
Skatole	24	0·2 (2/15)	0·6 (3/20)
Hexane thiol	2	0·8 (6/15)	0·7 (7/20)

The term 'faecal' was used mostly by the inexperienced group. Faecal is not well defined by any single *pure* chemical used. Note that skatole was further purified between Expts. I and II.

Fishy

Q (17/20, —). Well understood. Usually refers to fresh fish, but sometimes stale or smoked.

		Expt. II	
	Expt. 1	E	I
Hexylamine	66	2·9 (12/15)	3·9 (17/20)
Trimethylamine	23	3·2 (14/15)	3·3 (16/20)

Well defined by hexylamine and trimethylamine. Equally understood by both groups, but with high variance for the inexperienced group.

Floral

Q (16/20, —). Well understood. Light, pleasant flower smells—the unpleasant are excluded.

		Expt. II	
	Expt. 1	E	I
β-Ionone	17	2·7 (13/15)	2·2 (15/20)
Phenyl ethanol	42	2·7 (11/15)	3·1 (17/20)
Exaltolide	12	1·2 (8/15)	2·0 (14/20)

Adequately defined by phenyl ethanol and β-ionone. Used with equal frequency by both groups for many substances. Experienced subjects showed low variance for β-ionone and high variance for phenyl ethanol. (See also Fragrant.)

Foul, putrid, decayed

Q (16/20, —). Well understood. Unpleasant. Rotting organic material (plant and animal), sewers, and certain cheeses.

	Expt. I	Expt. II	
		E	I
Dimethyl propane thiol	9	2·5 (14/15)	2·5 (17/20)
Butyric acid	26	1·1 (8/15)	2·6 (16/20)
Diethyl sulphide	17	2·1 (12/15)	1·6 (13/20)
Dimethyl disulphide	11	1·7 (11/15)	1·0 (10/20)

Well defined for both groups by dimethyl propane thiol. The quality is equally used by both the experienced and the inexperienced groups, and is associated with substances showing other unpleasant but widely different qualities (e.g. sulphurous, rancid, cheesy). The variance for the experienced group was generally low, especially for dimethyl propane thiol.

Fragrant

Q (16/20, 40/46). Well understood. Mostly pleasant, fresh floral odours—perfumes, flowers and herbs.

	Expt. I	Expt. II	
		E	I
β-Ionone	22	2·7 (12/15)	2·9 (16/20)
Vanillin	26	1·8 (13/15)	3·1 (17/20)
Phenyl ethanol	9	2·2 (10/15)	3·0 (19/20)
Exaltolide	29	1·9 (12/15)	2·6 (19/20)
Methyl salicylate	0	0·7 (8/15)	1·4 (11/20)

Well defined by several substances, as indicated. Frequently used by both groups for many substances. Experienced subjects showed low variance for this quality with vanillin and very high variance with phenyl ethanol. The inexperienced group showed low variance with phenyl ethanol. Schutz (1964): reference standard for Fragrant–Methyl salicylate. Note that particularly for the experienced group all floral odours were fragrant but not all fragrant odours were floral.

Fruity (other than citrus)

Not included as such in questionnaire studies. See Fruity citrus, also lemony.

	Expt. I	Expt. II	
		E	I
Benzyl acetate	36	2·6 (13/15)	1·8 (11/20)
Hexyl butyrate	38 (fruity) 15 (ester/boiled sweets)	2·5 (12/15)	1·1 (8/20)

Well defined for experienced subjects by both stimuli, but far less so for the inexperienced. The experienced group showed very low variance for benzyl acetate.

Fruity (citrus)

Not used in questionnaire studies.

	Expt. I	Expt. II
Geranial	33	Not used

Not adequately defined by any single stimulus used in Expt. II: purified geranial was no longer available.

Garlic/onion. See Sulphurous.

In questionnaire as:

Raw vegetable (garlic/onion)

Q (15/10, —). Well understood. Mainly as garlic or onion, but also cabbage and salad vegetables; sharp and pungent. Pleasant or unpleasant according to context.

		Expt. II	
	Expt. I	E	I
Diethyl sulphide	52	1·8 (9/15)	1·5 (10/20)
Vinyl ethyl ketone	21	1·7 (8/15)	1·1 (7/20)
Dimethyl disulphide	20	1·1 (8/15)	0·7 (6/20)

Used predominantly by experienced subjects for whom variance was high for vinyl ethyl ketone and there was a strong correlation with sulphurous. Apparently not really well defined by any single chemical; a natural complex would probably be better.

Goaty

Q (—, 36/46). Moderately well understood. A general class of smells associated with various animals. Mostly unclean and associated with sweat, urine and excreta. Often unpleasant. Represents traditional hircine or caprylic class of odours. Schutz (1964) bracketed goaty with sulphurous as a reference standard defined by diethyl sulphide (0·03 per cent). In our experience this is erroneous. Goaty was rarely used in the experimental studies.

Greasy

Q (—, 23/46). Moderately understood. Machinery, also stale oil and fat. Not commonly used.

Green. See Herbal, green, cut grass, etc.

Q (—, 19/46). Not well understood. Mostly fresh cut grass, green leaves and stems.

Heavy

Not used in questionnaire studies.

		Expt. II	
	Expt. I	E	I
Dimethylpropane thiol	0	1·3 (6/15)	2·8 (15/20)
Hexane thiol	0	1·7 (8/15)	2·2 (12/20)
Guaiacol	0	0·7 (6/15)	2·0 (15/20)
Hexanol	0	0·5 (4/15)	2·1 (14/20)

Quality moderately well defined by thiols which also had high ratings for putrid and sulphurous. Used to a very much greater extent (for all but two stimuli) by the inexperienced group, who often placed it second or third in mean ratings. The inexperienced group showed low variance for this quality with guaiacol. Much less used by the experienced group.

Herbal. See Green and crushed green leaves.

Q (—, 40/46). Well understood. Mostly particular herbs (especially those used in cooking). Sweet to some.

		Expt. II (Herbal, green, cut grass, etc.)	
	Expt. I	E	I
Acetaldehyde	5	1·4 (10/15)	0·9 (6/20)
Coumarin	17	0·5 (3/15)	0·1 (2/20)

Not effectively defined by any single stimulus used especially for the inexperienced group. Perhaps a natural complex would be more suitable.

Honey-like

Q (—, 37/46). Moderately well understood. A sweet heavy odour, possibly distinct from the sweetness of sugar. Sickly to some. Not used in Expt. I. Only used in Expt. II as a single, spontaneous supplementary comment applied to phenylacetic acid and diacetyl.

Hyacinth

Q (13/20, —). Not well understood. Reference made to a specific, highly perfumed flower odour. Pleasant, velvety, overpowering, almost intoxicating. Sickly to some.

		Expt. II	
	Expt. I	E	I
Phenyl ethanol	3	Not included even as a spontaneous comment.	

Lemony. See Fruity (citrus)

Q (13/20, —). Not well understood. Mostly lemon or lemon juice and implying a taste. Tangy and sharp. Associated with lemon verbena shrub and lemon-scented herbs and grasses.

Light

Not used in questionnaire studies.

		Expt. II	
	Expt. I	E	I
Musk xylol	0	0·9 (4/15)	2·3 (12/20)
Ethanol	0	0·9 (7/15)	1·6 (12/20)
Acetaldehyde	0	0·9 (7/15)	1·3 (11/20)

Not well defined by any stimulus used in these studies. Used to a much greater extent by the inexperienced than the experienced groups, and for many more stimuli. Variance very high for this quality with musk xylol (inexperienced group) and low for acetaldehyde and ethanol (experienced group).

Mealy

Q (—, 38/46). Well understood. Mostly itself, illustrated in various ways, such as chicken feed, flour mills, bran. Not used in experimental studies.

Meaty. See Like blood, raw meat.

Q (13/20, 38/46). Moderately well understood. Includes both raw (butcher's shop) and cooked (roasted, stewed) meat odours.

Expt. I: Trimethylamine 7. In Expt. II the terms meaty (cooked) and raw meat (like blood) were available. They were used infrequently at a low level and only for selected amines and sulphur compounds. Therefore the meat odours were not well defined by any single stimulus used in these studies.

Medicinal

Q (15/20, —). Well understood; primarily associated with hospitals, surgeries and chemists' shops.

	Expt. I	Expt. II
Iodoform	38	The term medicinal not included in this study
Guaiacol	29	
Ethanol	29	
Methyl salicylate	27	

Metallic

Q (14/20, —). Moderately well understood. The smell of metal being polished, rusty metal, etc.

		Expt. II	
	Expt. I	E	I
Hexane thiol	2	0·5 (3/15)	1·1 (6/20)
α-Picoline	0	0·3 (2/15)	1·0 (6/20)
Ethyl benzene	0	0·3 (3/15)	1·1 (7/20)
Hexanol	0	0·5 (4/15)	0·4 (4/20)

Used more by the inexperienced group. Not adequately defined by any stimulus used. Schutz (1964): reference standard for Metallic—Hexanol.

Minty

Q (17/20, —). Well understood. Mostly garden mint or peppermint. Other associations cool and fresh.

		Expt. II	
	Expt. I (Minty, pepperminty)	E	I
Safrole	17	1·5 (9/15)	1·9 (14/20)
Menthol	15	1·8 (10/15)	1·2 (11/20)
Methyl salicylate	2	1·1 (9/15)	0·5 (7/20)

Best defined by the stimuli indicated. The experienced group showed low variance for this quality with methyl salicylate. Perhaps the term 'minty' is more applicable to natural complexes than to single chemicals.

Mothballs

Q (18/20, —). Well understood. Mostly in terms of naphthalene, mothballs themselves, old stored clothes, and occasionally camphor.

		Expt. II (Like mothballs)	
	Expt. I	E	I
Naphthalene	82	4·1 (15/15)	4·3 (20/20)
Indole	30	1·9 (11/15)	2·4 (13/20)
p-Dichloro-benzene	38	1·8 (12/15)	1·5 (10/20)
Skatole	35	1·5 (9/15)	1·3 (8/20)

Completely characterized by itself. Equal frequency of use by both groups over all stimuli. Low variance for this quality with indole and *p*-dichloro-benzene (experienced group).

Mouldy. See Earthy.

Musk

Q (—, 22/46). Not well understood, but otherwise correctly described as of animal or floral and herbal origin. Sometimes confused with musty.

		Expt. II (Musk-like)	
	Expt. I	E	I
Exaltolide	3	1·1 (6/15)	0·8 (7/20)
Musk xylol	0	1·0 (6/15)	0·4 (5/20)
Indole	0	0·5 (6/15)	0·8 (8/20)

Infrequently used by both groups. (As this quality was represented by two musk compounds, specific training is obviously required to identify it.)

Musty. See Earthy.

Q (18/20, —). Well understood. Associations include damp, dusty old books, cellars and uninhabited houses.

Oily

Q (16/20, —). Well understood, mostly in terms of oily machinery, but also greasy and edible oils, etc.

		Expt. II (Oily, fatty)	
	Expt. I	E	I
Hexylamine	11	0·8 (7/15)	2·4 (14/20)
γ-Butyrolactone	29	1·0 (8/15)	1·2 (10/20)
Diacetyl	8	1·9 (10/15)	1·4 (9/20)
Heptanol	14	Not used	

Best defined by hexylamine for the inexperienced group, who showed low variance for this quality, and by diacetyl for the experienced group. Used for many more stimuli by the inexperienced group. Schutz (1964): reference standard for Oily—Heptanol.

Overcooked vegetables

Q (10/20, —). Not well understood; otherwise correctly interpreted as unpleasant cabbage, burnt potatoes, second-rate boarding houses, etc. Deliberately included to cover the presence of sulphides in cooked vegetable aroma.

		Expt. II (Cooked vegetables)	
	Expt. I	E	I
Dimethyl disulphide	0	0·9 (9/15)	0·6 (4/20)
Diethyl sulphide	0	0·5 (6/15)	0·4 (5/20)

Not well defined by the stimuli used in these investigations.

Paint-like

Q (—, 37/44). Well understood, mostly in terms of paints based on turpentine and linseed oil.

		Expt. II	
	Expt. I	E	I
Ethyl benzene	22	2·0 (11/15)	2·5 (13/20)

Paint-like adequately defined by this stimulus.

Peppermint. See Minty.

Q (17/20, —). Well understood in terms of confectionery; also termed clean, 'minty', etc.

Petrol-like

Q (—, 44/46). Well understood, mostly in terms of itself. Thirty-one references made to petrol, garages, cars, etc. Also sweet. Some trigeminal features.

	Expt. I		Expt. II (Like petrol, solvent)	
	Petrol	Chemical solvent	E	I
Ethyl benzene	31	40	3·6 (15/15)	2·9 (17/20)
Furan	20	4	2·3 (11/15)	1·6 (11/20)
2-Propanol	2	21	1·2 (8/15)	1·4 (9/20)

Well defined for both groups by ethyl benzene. Quality used more, and with lower variance, by the experienced than by the inexperienced group.

Pungent. See Sharp.

Q (16/20, —). Well understood as a form of irritation. Described as sharp, eye-watering, penetrating, stinging, etc. Sometimes confused with putrid.

Rancid

Q (15/20, 44/46). Well understood. A sharp sour smell, especially that of old stale butter or other fats.

	Expt. I	Expt. II E	Expt. II I
Butyric acid	36	1·9 (11/15)	2·4 (14/20)
γ-Butyrolactone	22	2·1 (12/15)	1·3 (9/20)
Diacetyl	8	1·3 (9/15)	1·4 (9/20)

Reasonably well defined by butyric acid for both groups. However, γ-butyrolactone fitted the definition much better for the experienced group in view of higher mean and low variance. Used more often by the inexperienced than by the experienced group. Schutz (1964): reference standard for Rancid—Butyric acid.

Raw vegetables. See Garlic/onion.

Sharp, acid, pungent

Sharp. Q (—, 37/46). Well understood; mostly an acid type of smell with varying degrees of pungency or other trigeminal sensation. Only two respondents referred to taste rather than smell.
Acid. Q (—, 30/46). Moderately well understood. Two types of response: (*a*) sharp, acid fumes; (*b*) sour, unripe taste.
Pungent. Q (16/20, —). Well understood as a form of irritation. Described as sharp, eye watering, penetrating, stinging, etc. Sometimes confused with putrid.

	Expt. I			Expt. II (Sharp/acid/pungent)	
	Sharp	Acid	Pungent	E	I
Ammonia	15	8	23	2·2 (11/15)	2·4 (15/20)
2-Propanol	7	2	5	1·5 (12/15)	1·2 (11/20)
Vinyl ethyl ketone	7	2	14	1·4 (10/15)	1·8 (12/20)
Phenyl ethanol	5	2	2	1·1 (10/15)	1·4 (9/20)
Ethanol	7	2	0	1·3 (10/15)	0·8 (7/20)
Acetaldehyde	3	15	3	1·1 (9/15)	0·9 (7/20)

Sharp and pungent both well defined by ammonia.

Sickly

Q (18/20, —). Well understood, but two distinct meanings possible. (*a*) Excessively sweet, heavy, floral odours including decaying flowers. (*b*) A second group including nausea, over-rich fat, sour milk, silage.

	Expt. I	Expt. II E	Expt. II I
γ-Butyrolactone	5	1·7 (11/15)	2·8 (18/20)
Hexylbutyrate	5	0·3 (2/15)	2·4 (15/20)
Pyridine	5	1·1 (9/15)	1·8 (13/20)

Well defined by γ-butyrolactone for the inexperienced group, especially in view of the low variance involved; inadequately defined for the experienced group by this stimulus. This quality was used much more by the inexperienced group.

Smoky. See Burnt.

Q (18/20, —). Well understood, various examples include burning wood (bonfires) and to a lesser extent coal, stale cigarettes. Not specifically unpleasant. Sometimes associated with acrid.

Smooth

Q (8/20, —). Rarely used spontaneously with odours. A special technical term sometimes used in the tobacco industry.

Soapy

Not used in the questionnaire studies.

		Expt. II	
	Expt. I	E	I
Octanol	16	2·3 (9/15)	2·4 (12/20)

Well defined but with high variance by octanol for both groups.

Sour, acid, vinegar, etc.

Sour. Q (—, 38/46). Well understood: mostly sour milk, rancid or cheeselike. Several references to vinegar or weak acids.

Acid. Q (—, 30/46). Moderately well understood, two types of response: (*a*) sharp, acid fumes; (*b*) sour, unripe taste.

Vinegar. Not used in questionnaire studies.

	Expt. I			Expt. II (Sour, acid, vinegar)	
	Sour	Acid	Vinegar	E	I
Diacetyl	9	8	25	1·1 (8/15)	1·7 (12/20)
Butyric acid	6	0	0	1·3 (10/15)	1·3 (8/20)
Phenyl acetic acid	10	2	5	0·8 (7/15)	1·3 (9/20)

Not well defined by the stimuli used in this experiment.

Spicy

Q (—, 38/46). Well understood. Usually as of spices. Two types associated (*a*) with rich cakes, etc. (often during cooking), flavoured with cinnamon, nutmeg, cloves, etc., and (*b*) with the sharp smells of pepper and curry.

		Expt. II	
	Expt. I	E	I
Eugenol	9 (spicy) 43 (cloves)	2·7 (12/15)	1·8 (10/20)
Safrole	7 (spicy) 48 (aniseed)	1·3 (7/15)	0·5 (7/20)
Benzaldehyde	0	0·3 (3/15)	0·4 (5/20)

Well defined for the experienced group by eugenol. One of the few qualities rated higher by the experienced than the inexperienced group. The odours of these stimuli may be too specific for them to be referred to generally as spicy. Eugenol had 10 additional comments associated with cloves/dentist; safrole 10 references to aniseed, and three to cough mixture. Schutz (1964): reference standard for Spicy—Benzaldehyde.

Sulphurous

Q (—, 44/46). Refers to odours of the sulphur dioxide, hydrogen sulphide type, bad eggs, industrial fumes and fog, burning sulphur and matches, etc. Trigeminal properties often noted. Reference to stale cauliflower, cabbage, etc., draws attention to link with (over)cooked vegetables.

		Expt. II	
	Expt. I	E	I
Dimethyl propane thiol	43	4·0 (14/15)	2·5 (13/20)
Dimethyl disulphide	7	1·9 (10/15)	0·2 (3/20)
Diethyl sulphide	10	2·8 (13/15)	0·9 (6/20)

Well defined, especially for experienced subjects, by dimethyl propane thiol, but the inexperienced group showed high variance for this substance. One of the few qualities generally rated higher by the experienced group than by the inexperienced: also used for more stimuli by the former. A close association between sulphurous and garlic/onion was evident for the experienced subjects. Schutz (1964): reference standard for Sulphurous—Diethyl sulphide.

Sweaty

Q (18/20), —). Well understood. Essentially offensive, acrid body odours, stale perspiration, etc.

		Expt. II	
	Expt. I	E	I
Phenyl acetic acid	5	0·9 (9/15)	1·7 (13/20)
γ-Butyrolactone	5	1·3 (11/15)	1·5 (12/20)
Butyric acid	0	1·5 (13/15)	1·4 (10/20)
Trimethylamine	7	0·4 (4/15)	1·0 (10/20)

Well defined for the experienced group by butyric acid. The inexperienced group showed low variance for this quality with trimethylamine.

Sweet

Q (18/20, —). Well understood. Complex, ranging from delicate floral sweetness to a heavy, cloying and sickly sweetness. Many examples given.

		Expt. II	
	Expt. I	E	I
Vanillin	29	4·3 (15/15)	4·2 (19/20)
β-Ionone	32	2·8 (14/15)	2·9 (19/20)
Exaltolide	24	1·6 (12/15)	2·6 (18/20)

Sweet is very well defined by vanillin. It is the most frequently used of all the terms and is used equally by both groups. Schutz (1964): reference standard for Sweet—Vanillin.

Throaty

Q (7/20, —). Infrequently understood or used.

Toasted

Q (15/20, —). Well understood. Mostly the odour of toasted bread. Mainly pleasant. Associated words: sharp, slightly burnt, smoky, warm and sweet. Not used in the experimental studies.

Vanilla-like

Q (12/20, 36/46). Moderately well understood. Confectionery examples, mostly containing vanilla flavouring or to a less extent almond flavouring. Often associated with sweet and sickly. Not always pleasant, especially when in high concentration or noticeably synthetic.

		Expt. II	
	Expt. I	E	I
Vanillin	26 16 (confectionery)	4·3 (15/15)	4·2 (19/20)
Benzaldehyde	3	1·1 (9/15)	1·0 (11/20)

The term tends to be specific; almond is more often referred to as vanilla-like than vanillin as almond-like.

Warm

Q (10/20, 12/46). Not well understood. Very varied meanings given, associated with human beings, hot air, warm buildings, hot bread, London underground, etc.

		Expt. II	
	Expt. I	E	I
Benzaldehyde	0	0·4 (3/15)	1·4 (13/20)
Vanillin	7	0·8 (6/15)	1·3 (10/20)
Diacetyl	6	0·5 (4/15)	1·1 (8/20)

Not well defined by any single stimulus used. Applied by the inexperienced group to all but five stimuli, with very low variance with benzaldehyde. The experienced group rarely used the term.

Woody

Q (17/20, —). Well understood, newly sawn wood, sawdust, etc.

		Expt. II (Woody, resinous)	
	Expt. I	E	I
Guaiacol	6	0·9 (8/15)	0·8 (9/20)
Cyclohexanone	6	0·4 (4/15)	1·0 (11/20)

Not well defined by any stimulus used in these studies.

Yeasty

Q (—, 37/46). Moderately well understood. Various examples include new bread and fermentation products generally. Not used in the experimental studies.

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A STUDY OF TWO FACTORS WHICH AFFECT AROUSAL LEVEL AND THE APPARENT DURATION OF A TEN-MINUTE INTERVAL

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Interview studies of aircrew suggested that the apparent duration of an interval during flight might be affected both by the demand made on the individual by his allotted task and by the amount of background distraction to which he was exposed. In a laboratory experiment, eight pilots were isolated for four intervals of 10 min. During two of the intervals the pilots were required to perform a simple tracking task, whilst in the other two they were not required to do anything; on one occasion for each task condition they were exposed to distracting stimulation. The palmar skin resistance was measured every ¼ min. during each 10 min. interval; afterwards the subject estimated its duration and indicated how alert he had been during it. The apparent duration was increased by the presence of distraction and decreased by the performance of the task; however, the effects of distraction and performance of the task on alertness and skin conductance were similar, both being in the direction of an increase in arousal. It was found that arousal changes could not account for more than an insignificant proportion of the observed alterations in apparent duration. It is concluded that an explanation in terms of the way in which attention is organized under the particular conditions of isolation obtaining is both consistent with the findings and may be a fruitful line for future investigation.

Interview studies of aircrew (Dearnaley, 1960; Gedye, 1960; Aitken, 1962) have indicated that under conditions which occur in the normal course of flying duty, the amount of time elapsed since some particular reference event may be wrongly estimated. This may lead to mistakes of considerable practical importance.

Some idea of the magnitude of the distortions which can occur in flight was obtained from the results of a questionnaire survey. Eighty-five fighter pilots were asked to indicate the longest and shortest apparent duration of a 10 min. interval which they had experienced during defined types of sorties (Aitken, 1965). The mean shortest apparent duration was reported during 'exciting' sorties (4·8 min.) and the mean longest apparent duration during 'boring' sorties (21·7 min.).

From the results of these studies, the two most important factors present in flying situations which affect the ability of aircrew to make accurate estimates of elapsed time seemed to be: (1) the demand made on the individual by his assigned task; (2) the amount of irrelevant stimulation to which he was subjected when performing the task.

Although factors which affect the apparent duration of time intervals have been extensively studied, none of the previous investigations appears to have been inspired by such reports. It is difficult to judge their relevance to practical problems without first undertaking fresh experimental investigations more directly based on the practical situation. It was decided, therefore, to try to produce changes in the apparent duration of an interval by manipulation of the variables which seemed to be important in flying, and in particular to assess to what extent alterations in 'level of arousal' could account for changes produced, since the interview studies had suggested that this might be important.

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METHOD

Eight pilots were each isolated in a sound-insulated room for four intervals of 10 min. The subject could talk to the experimenter by means of an intercom, but otherwise had no access to events outside the room.

The experimental factors, which will be referred to briefly as 'task' and 'distraction', were produced as follows.

Task. The subject sat on a turntable, mounted on which was a simple two-channel compensatory tracking task. The subject was required to correct deviations of the pointers from the central position by the corresponding control knob. The pointers followed a sinusoidal excursion of 20° amplitude with a period of 2 min., and were 180° out of phase. The values obtained of the integrated modulus misalignment were examined, but as no conclusions of relevance were drawn, they are not discussed further.

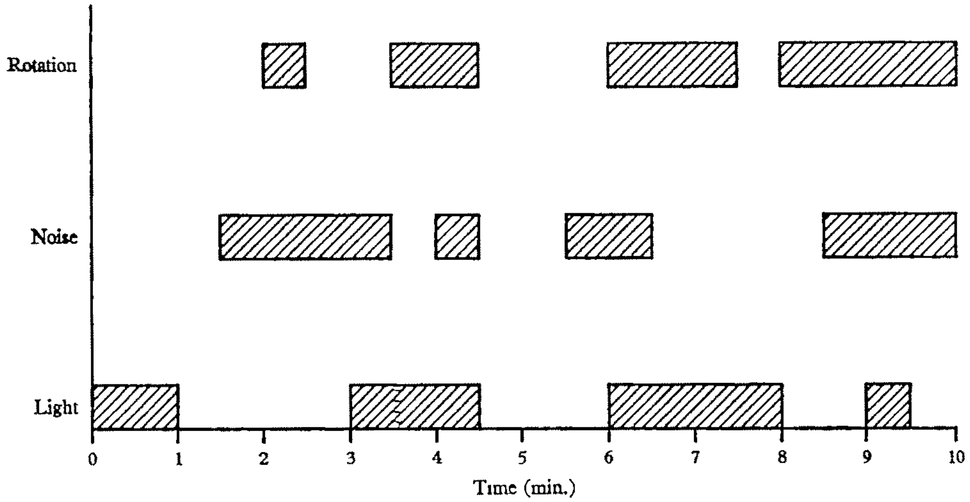

Fig. 1. An example of a pattern of distraction used in the experiment. The hatched bars indicate that the stimulus to which they refer was 'on'.

Distraction. Stimulation was given in three different ways: (i) by having the room in light or in darkness; (ii) by having the room quiet or noisy: mounted on the turntable behind the subject was a vibrator which acted as a noise source; (iii) by having the subject stationary or rotating (the turntable could be rotated at an angular velocity of about 40° per sec.) For each modality the stimulus was 'on' for four periods of $\frac{1}{2}$, 1, $1\frac{1}{2}$ and 2 min. and 'off' for four similar periods. Orders of these eight periods were allocated to subjects at random in a design balanced independently for each kind of stimulation. This arrangement provided a background of unpredictable irrelevant events while preserving, on average, a uniform amount of stimulation at any chosen moment during the 10 min. interval. An example of the pattern of stimulation is shown in Fig. 1.

Design. The experimental session for the first four subjects (group A) consisted of four intervals of 10 min. Successive intervals were separated by 5 min. in contact with the experimenter, marked by a red light. The second four subjects (group B) had an additional calibration interval preceding the four experimental intervals, as will be explained later.

A factorial design was used so that each subject experienced four treatments (see Table 1); each group of four subjects formed a 4 × 4 Latin square, subjects being allocated at random.

Table 1. *Factorial design*

Treatment	Factors: Distraction	Factors: Task	Symbol
I	Absent	Absent	D_0T_0
II	Absent	Present	D_0T_1
III	Present	Absent	D_1T_0
IV	Present	Present	D_1T_1

Measurements

Subjective procedures

Apparent duration. Each subject was given paper on which there were four 25 cm horizontal lines; he was instructed to regard each line as a linear representation of elapsed time, the extreme left-hand end representing the beginning of the interval of isolation, and to mark appropriately. This method of estimation was used in order to avoid the influence of the conventional divisions of the clock face, which is one of the disadvantages of verbal estimation (Yerkes & Urban, 1906).

The four subjects in group A were presented with lines marked every 25 mm and labelled in minutes from 0 to 50, so that the scale was 0·2 min. per mm. The four subjects in group B were presented with similar lines marked at a point 100 mm from the left-hand end. They were told that this interval represented the standard time interval as demonstrated to them by the interval of isolation under basal conditions which preceded the four experimental intervals. This calibration interval lasted 10 min., so that the scale for the second group of subjects was equivalent to 0·1 min. per mm. At the end of the session each subject in group B estimated the duration of the standard interval, expressing the result verbally to the nearest minute. His estimates for the four experimental intervals were then expressed in minutes.

Level of alertness. Each subject was given paper with four 100 mm horizontal lines. They were instructed to regard each line as a representation of the various possible degrees of alertness, ranging from a state of sleep at the extreme left, through a region signifying moderate wakefulness in the middle, to a state of extreme alertness at the right. The subject marked the line to indicate how he had generally felt during each interval. The scores were expressed on a scale ranging from -1 (extreme left) through 0 (moderate alertness) to $+1$ (extreme right). This particular convention was chosen in order to emphasize the centre of the line as the most relevant anchor point.

Objective procedure

Skin resistance. The resistance between an electrode on the palm of the left hand and a reference electrode on the left leg was measured every $\frac{1}{2}$ min. throughout each interval. The palmar electrode was of dry silver-plated copper gauze; it was dome-shaped and was retained with a rubber strap. The area of contact with the skin was about 1 cm^2. This arrangement allowed free circulation of air around the electrode and prevented pooling of sweat. The reference electrode was a silver plate moistened with saline jelly. The measurement of resistance was made using a bridge circuit, with an almost constant current of 7 μA d.c. through the skin.

As the appropriate measure for study appears to be conductance, readings were transformed to conductance in micromhos by taking the reciprocal of the resistance in megohms.

Experimental procedure

Each subject was told that the purpose of the experiment was to determine the most acceptable way of spending a short interval in isolation. Subjects were then deprived of their watches, so that they had no access to 'local time'. For subjects in group A no further reference to time was made. However, subjects in group B were asked to try not to count anything whatsoever, not to try to maintain an estimate of elapsed time as they went along, and not to modify their final estimates in accordance wth any preconceived ideas about possible sources of error in estimation. Group B subjects were also told that the four intervals would be of different duration, and, in order to avoid their necessarily thinking in conventional time units, the design of the experiment was modified so that they were given an initial calibration period of 10 min. isolation. They were told to regard the duration of this period as a standard time interval and to make their estimates of the duration of the following intervals in relation to it.

This modified technique for subjects in group B was introduced when it became clear that the subjects in group A were going about the matter in widely differing ways. While not performing the task, subject 1 counted his respiratory cycles and subject 2 counted his pulse beats; subject 4 made allowance for a preconceived notion that time would pass more slowly when unoccupied.

RESULTS

Subjective measures

Duration scores. Initially the results for groups A and B were examined separately. Since in group A the scores were expressed directly in minutes, no transformation was required before analysis. The estimates of the duration of the standard interval from the subjects in group B were 17, 10, 8 and 12 min. respectively; the scores in standard units were transformed to minutes by using conversion factors derived from these calibrations. Analysis of variance showed no difference between groups A and B, so the results were pooled. In both groups subjects differed among themselves (for group A, $P = 0{\cdot}05$; for group B, $P = 0{\cdot}001$). There was no difference attributable to order of presentation. There was no significant interaction between the two experimental factors, and both produced significant main effects (see Table 2). It can be seen that the mean score with task is *lower* than the mean score without task, while the mean score with distraction is *higher* than the mean score without distraction.

Table 2. *Main effects of task and distraction*

(Each average is the mean of 16 values, two from each of eight subjects.)

	Factor	Absent	Present	Difference	*P*
Mean duration score (min.)	Task	12·2	10·3	−1·9	0·01
	Distraction	10·2	12·4	+2·2	0·001
Mean adjusted* duration score (min.)	Task	12·7	9·8	−2·8	0·01
	Distraction	10·4	12·1	+1·7	0·025
Mean alertness score: range −1 to +1	Task	−0·42	−0·11	+0·31	0·001
	Distraction	−0·35	−0·18	+0·17	0·01
Mean change in log conductance	Task	−0·0764	−0·0200	+0·0564	0·05
	Distraction	−0·0692	−0·0272	+0·0420	0·05

* Adjusted for covariance with alertness scores.

Alertness scores. Analysis of variance showed no difference between groups A and B, so the results were pooled. In both groups subjects differed amongst themselves (for both groups, $P = 0{\cdot}001$) and there were significant differences with order of presentation ($P = 0{\cdot}01$). The mean score for the first two intervals was $-0{\cdot}38$, and for the second two $-0{\cdot}15$, so that subjects appeared, in general, to be more alert in the latter half of the experiment. There was no significant interaction between the two experimental factors, and both produced significant main effects.

It will be seen from Table 2 that the mean score with task is *higher* than the mean score without task, and that the mean score with distraction is *higher* than the mean score without distraction. All the means are negative, which implies that alertness during the experiment was generally below 'moderate wakefulness'.

Objective measure

Skin conductance. The skin conductance values obtained every ½ min. were plotted against time for each subject separately; most appeared either to remain fairly constant or to fall in a roughly exponential manner. Overall increases in conductance were infrequent, though an initial rise was observed. This has been previously reported as usual in such circumstances (Cattell, 1928).

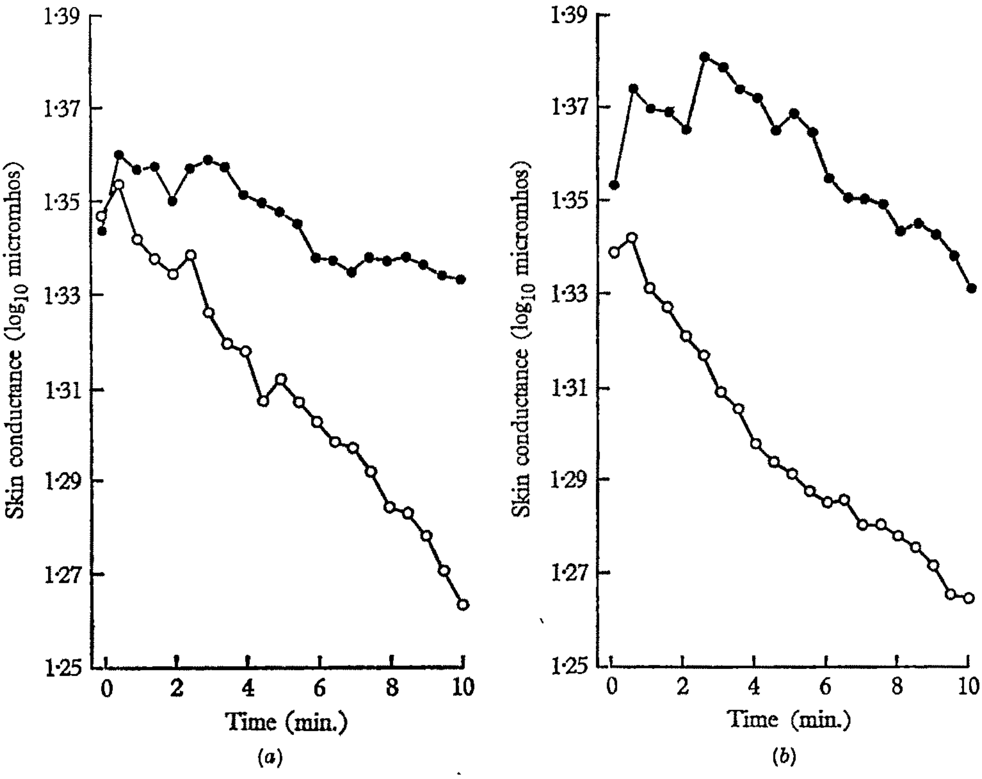

Fig. 2. Average log conductance levels ($\log_{10}$ micromhos) every ½ min. throughout the intervals of isolation under various conditions in the presence (●—●) and absence (○—○) of (*a*) 'task' and (*b*) 'distraction'. Each point is the mean of 16 values, two from each of eight subjects.

The problem of choosing an appropriate measure for the quantitative analysis of skin resistance has recently been discussed by Lader & Wing (1966), who argue that such a measure must be based on an understanding of the physiological mechanism responsible for the resistance and its fluctuations. It has been demonstrated that the amount of palmar sweat produced (Darrow, 1934*a*, *b*) and more recently the number of sweat glands in a unit area (Thomas & Kerr, 1957) have a rectilinear relationship to the reciprocal of resistance, conductance. All the available evidence points to the resistance being dependent on the number of active sweat glands (Lader & Montagu, 1962), which behave as a set of resistances in parallel; this suggests conductance as the valid measure, since parallel conductances are additive (Freeman & Katzoff, 1942). As this transformation gives a measure which reflects the number of sweat glands tonically active at any given moment, any increase will be related to the proportion of sweat glands activated by the inducing stimulus. As it is this alteration which is under study, a better appreciation of the data is obtained by examining the change in relation to the original level by means of a logarithmic transform.

Since interest was focused on the alteration occurring in skin resistance during the intervals, change in log conductance in unit time is logically the most appropriate measure for assessment. The mean log conductance after 0, 0·5 and 1 min. and 9, 9·5 and 10 min. isolation were calculated for each subject and each condition separately. The differences between them were obtained on which analysis of variance was carried out. This showed no significant differences between groups A and B, so the results were pooled. There was no difference between subjects, suggesting that the measure used had successfully eliminated between-subject differences. There was no significant interaction between the two experimental factors and both produced significant main effects (see Table 2).

Both task and distraction *decrease* the rate of fall of conductance to about the same extent. In Fig. 2*a*, *b* the mean values for each comparison are shown plotted against time. It will be seen that the approximation to a linear decay with time is best for conditions without task and distraction, suggesting that one of the effects of the factors was to delay the onset of the decay.

General observations

Subjects had little difficulty in accepting the techniques of subjective assessment which were used; in particular, the notion of arousal as a continuum from sleep to extreme wakefulness appeared to relate to a familiar aspect of their everyday experience. This is borne out by the existence in aircrew vocabulary of a number of words and phrases which specifically relate to various aspects of the concept of arousal.

An interesting distinction was made between arousal in the sense of *alertness*, ranging from 'sleep' to 'frantic activity' (not necessarily implying fear), and arousal in the sense of *anxiety*, ranging from 'relaxed' to 'panic'. As used here the words appear to be part of normal aircrew vocabulary. However, it was clear that the continuum used in rating arousal in the present experiment was the former, and that the answers did not imply any particular affective state.

Discussion suggested that, during isolation, subjects in general preferred 'something' rather than 'nothing'; it appeared that the stronger the feeling about the issue, whether for or against, the longer the apparent duration of the interval. A similar relation was suggested by the results of another experiment (Aitken *et al.*, 1963).

DISCUSSION

Skin conductance as an indicant of arousal

Following the discovery of structures in the brain stem responsible for alterations in the state of wakefulness (Moruzzi & Magoun, 1949), it became customary to use the word 'arousal' to refer to the level of activity in these structures, or in other words, to the degree of *activation* of the postulated arousal mechanism.

Unfortunately, it has since been shown that dissociation of the activation pattern of the electroencephalogram and the behavioural manifestations of arousal may occur spontaneously, particularly when the level of arousal is changing (Hess *et al.*, 1953), and may be induced by some drugs (Wikler, 1952; Bradley & Elkes, 1957). It is not, therefore, safe to assume that activity in these structures always reflects the degree of arousal of the individual in the behavioural sense of the term. Lindsley (1957) has suggested that the word 'arousal' be used primarily in a behavioural sense, which avoids these difficulties.

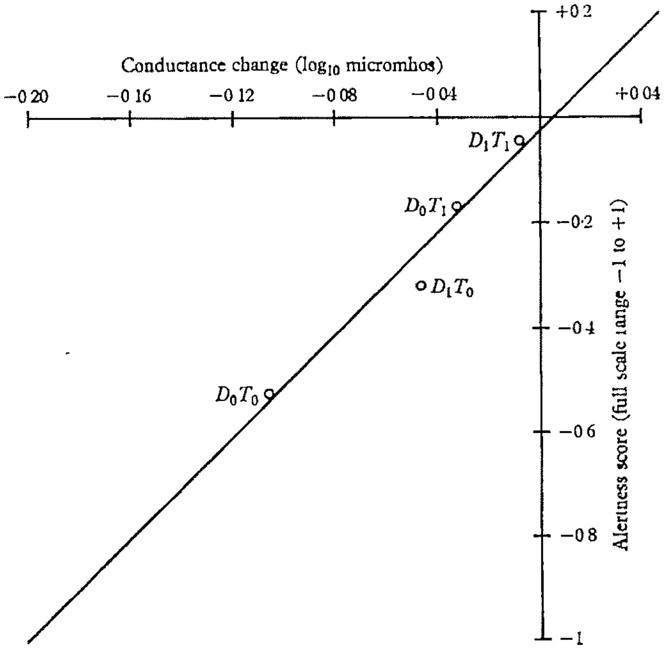

Fig. 3. The relation between log conductance change and alertness score. The regression shown plotted is $y = 4\cdot83x-0\cdot03$ ($P = 0\cdot025$). D_0 = no distraction; D_1 = distraction; T_0 = no task; T_1 = task.

In this discussion the word 'arousal' will be used to refer primarily to the subjective state of alertness, so that a measure of arousal is by definition a quantification of this. Levels of activity in neural structures which are believed to subserve this state, and in the body generally, will be regarded as ***indicants*** of the individual's level of arousal insofar as they can be shown to be capable of providing a reliable basis for predicting the level of alertness. In the present context this means that the derived measure of change in log conductance can be regarded as an indicant of arousal if it can be used to predict the alertness score with any reliability.

Examination of Table 2 shows that both task and distraction increased the alertness scores and produced less decrease in conductance during the intervals of isolation. It was found that the relation between alertness score and skin conductance could be expressed by a regression equation determined from means of the scores for the four experimental conditions. This is shown in Fig. 3.

It can be seen that the regression line cuts the x and y axes at a point very close to the origin, so that no change in conductance corresponds to moderate alertness. Extrapolation gives a value of $-0{\cdot}22$ log micromhos as the change in conductance corresponding to a state of sleep (alertness score $= -1$), indicating a rate of fall of 0·022 log micromhos per min.

It has been known for some time that palmar conductance is low during sleep (Waller, 1919; Farmer & Chambers, 1925). Richter (1926) found that changes in palmar conductance while falling asleep closely followed the depth of sleep, the rate of change being related to the depth ultimately reached. He also noted that states of drowsiness were accompanied by falling conductance both in normals (Richter, 1928) and in narcoleptics (Richter, 1929*a*), where a rapid fall in conductance occurred before they fell asleep after being left undisturbed. However, his only published result which can be compared directly with this experiment is a single recording from a spider monkey (Richter, 1929*b*) trained to sleep in an infant's cot for periods of about 6 hr. The rate of fall of conductance in this animal, assessed over the first 30 min. while falling asleep, was 0·018 log micromhos per min., a value which is of the same order as the one obtained by extrapolation from our results (0·022 log micromhos per min).

Assuming a more or less constant initial level of alertness, corresponding to the centre of the alertness scale, the simplest explanation for the relation between conductance change and alertness score found in our experiment would be that during intervals of isolation subjects were falling asleep at different rates, depending on the conditions; and that their indications of average level of alertness reflected these different rates, since the greater the fall in level of alertness, the lower the average level of alertness.

Taken together, these findings suggest that it might be profitable to think of an individual spending most of his waking life at a certain normal level of arousal adapted to the requirements of moment-to-moment existence, and corresponding to the centre of the alertness scale we have used. Conditions conducive to sleep initiate a progressive fall in level of arousal below this normal level, whereas invigorating conditions increase it suddenly above the normal level, and are followed by a return to the normal level. This implies a different picture for increases of arousal above normal level than for decreases of arousal below normal level.

Arousal and apparent duration

It appears, therefore, that both task and distraction reduced the rate of fall of arousal of the subjects, but produced opposite effects on apparent duration. It follows that these effects on apparent duration cannot be completely accounted for by arousal changes, and it is thus necessary to decide to what extent the changes can be ascribed to changes in arousal. An attempt to answer this question was made by means of an analysis of covariance between the duration scores and the alertness scores, but this failed to reveal a significant regression. In order to demonstrate this lack of effect of making an allowance for such alteration in arousal the adjusted duration scores, using a correction factor derived from the regression, are also shown in Table 2.

It may be concluded, therefore, that arousal changes play only a very small part, if any, in the production of the main effects demonstrated in this experiment. At the same time it must be emphasized that this does not mean that arousal changes might not be important in other circumstances, particularly those leading to states of hyper-arousal.

Organization of attention and apparent duration

Since arousal changes cannot account for the effects of task and distraction on apparent duration, it is necessary to find an alternative explanation. The circumstances of the experiment suggest that the way in which the subject's attention is organized by the conditions of isolation might be important. Explanations of this kind have often been put forward in the past; for example, Sturt (1923) suggested that the more focused a person's attention, and the fewer the changes of attention, the shorter a given interval would appear. This view is consistent with the present results provided that it is accepted that attention is likely to be more focused, and undergo fewer changes, during the performance of the task; and be more diffusely organized, and undergo more changes, during the conditions of distraction; and that these opposing factors will cancel each other out when the task is performed in conditions of distraction.

If an explanation on these lines is correct one might expect the effect of a factor on apparent duration to be related to an individual's attitude towards it, since attitude may influence the organization of attention. One might, for example, predict that the shortening effect of the task would be greatest in those who preferred performing to not performing, and the lengthening effect of distraction would be greatest in those who preferred being distracted to not being distracted. The information available to confirm or refute this suggestion is sparse as unfortunately the design adopted does not allow the examination of subject × factor interactions. Some scores were obtained on which to judge attitude, but the number of subjects was too few and no firm conclusions could be drawn.

Any comprehensive theory which is put forward to explain the effects observed in this experiment must be consistent with what is known about the way in which the estimates were made. From the fact that the same effects were observed for subjects in both groups A and B, who in general adopted differing methods, it would appear that differences of this kind are not likely to play a large part in any theory to account for the observed results. Similar conclusions about a slightly different situation were reached by Gilliland & Martin (1940) and Gilliland & Humphreys (1943), who found that while most people spontaneously adopt some kind of counting procedure in experiments on apparent duration, their estimates are as accurate when they are asked not to count. However, with practice they are able to reduce the variability of their performance by counting, so that it can provide a useful aid in certain circumstances.

CONCLUSIONS

The main aim of this experiment was to discover whether or not it was possible to produce, under laboratory conditions, the kind of distortion of the apparent duration of 10 min. intervals that had been reported by pilots in flight. The results confirm the importance of the factors 'task' and 'distraction' in producing distortions of the apparent duration of time, but they indicate the possibility that the effects of these factors in individuals may depend to some extent upon his attitude towards them.

While the possibility of arousal changes induced by the factors task and distraction being important under other circumstances cannot be ruled out, there is no evidence that they were important in this experiment; an explanation in terms of the way in which the subject's attention was organized under the experimental conditions might be more promising. It would appear that an exploration of the effects of different kinds of distraction with a view to making such phrases as 'diffuseness of organization of attention' more precise, and gaining evidence on which to build an 'attentional' theory, would be one of the most profitable approaches for further study.

The authors would like to thank Wing Commander T. C. D. Whiteside for encouraging them to undertake an experimental investigation of this problem; Dr A. J. Benson for his encouragement and advice on the construction of the apparatus; Miss Helen M. Ferres for help with the statistical design of the experiment and Mr P. Freeman for help with the analysis of the results. They would also like to acknowledge the assistance of Mrs Amelia R. W. Coupe and Mrs Margaret L. Perry in carrying out the experiment.

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LOAD AND PUPILLARY CHANGES IN CONTINUOUS PROCESSING TASKS

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The interaction of rate of presentation, and number of transforms and possible responses was examined in connexion with the effect of cognitive load upon pupillary dilation. The task involved continuous processing of auditorily presented material. The two criteria of task difficulty both contributed to raising pupillary diameters, which were further increased at the moment of button-press responding. That variations in level arousal were involved was further borne out by the tendency for certain regular changes to occur in pupillary dilation in the course of the processing tasks.

Arousal and attention are significant as intervening variables in such information-processing tasks as problem solving, reaction time, vigilance and probability estimation. Lacey (1967) questions the degree to which the various autonomic and electrophysiological indices may interact and correlate. Despite evidence in some situations of low or even negative correlations between the various measures, together with inter-subject, inter-task and inter-measure idiosyncrasies, it is important that any such index should be thoroughly documented, if only to provide data from which further conclusions can be drawn. The pupillary index is one such measure which is currently receiving renewed interest.

Gibson & Hall (1966) and Costello & Hall (1967) related acceleration in heart rate to performance of mental tasks under conditions of white noise. Blatt (1961) found that such elevations tended to occur at significant points during solution. On the other hand, Glass (1966) found no evidence of a differential effect on the blocking of the occipital α-rhythm with easy and difficult mental arithmetic tasks. As very strong evidence was found by the author (Bradshaw, 1967), using pupillary measures and considerably more differentiated levels of task difficulty, it is felt that one reason for the failure of Glass lay in his choice of material; there seemed relatively little difference between his easy and difficult tasks. This indicated the need for improved criteria of cognitive load.

Indeed, Hess (1964), Kahneman & Beatty (1966) and Bradshaw (1967) in a variety of experiments demonstrated the effect of problem difficulty in increasing pupillary diameters. The tasks included mental arithmetic and short-term memory, single- and multiple-solution anagrams, and cartoon stimuli, with responses including immediate, stored, verbal and button-press. Thus, while there may well be some doubt as to the relevance of changes in pupil sizes as an accurate indicator of the direction of affect in emotive situations (Loewenfeld, 1966; Bradshaw, 1967), with cognitive tasks it is perhaps now a matter of documenting the extent and direction of such changes with reference to the various conditions imposed.

The purpose of the experiment reported here was to determine the effects of using auditory input of material in continuous processing tasks of the Wittenborn (1943) 'attention' type, with responses to be made by button-press alone. Indeed, one problem when comparing responses to tasks held as varying in cognitive difficulty has long been that of quantifying such levels of difficulty. The advantage of using continuous-processing tasks is that a number of easily manipulated variables offer themselves. Briefly, the cognitive load can be determined by two factors: the rate of presentation and the general complexity of processing requirements. The latter can be made a simple function of the number of input items for simultaneous processing or comparison, the possible and actual number of required outputs or responses, and the number of comparisons or transforms necessary before action is taken. Consequently, it was predicted that there would be greater overall pupillary dilations to a faster rate of presentation and to material requiring more decisions and a greater range of motor responses.

METHOD

Apparatus. A Bolex 16 mm reflex ciné camera filmed the left eye of the subject, at a lens-to-eye distance of some 8 in., the subject's head being firmly positioned in an adjustable head-holder. Both eyes were unimpeded in their view of an illuminated back-projection screen, with a central fixation point, 56 in. distant. The camera was fitted with a 75 mm telephoto lens, 5 mm extension ring, and 3 dioptre supplementary lens. The complete eye, corner to corner, exactly filled one frame, reproducing at one-third life-size on film. The camera was externally driven at 160 frames per min.

The eye was illuminated by two small 25 W bulbs, both 12 in. distant from the eye, and situated to the left of the camera, above and below it. The angle subtended on the eye between either lamp and the line of fixation was 45°, with 45° between the two lamps. This provided a tangential illumination of the eye equivalent to 13 ft.-lamberts, as measured by reflectance from a standard white surface, and was kept constant by a voltage stabilizer. High-speed Ilford HPS film was uprated to a film speed in excess of 5200 ASA by a prolonged dilute developing technique. Developed film was pulled through a modified still projector with a short wide throw and projected on to graph paper, such that 1 in. projected was equivalent to 1 mm on the eye. Measurements were recorded to the nearest $\frac{1}{10}$ in., or 0·1 mm at the eye.

Material was taped and presented via earphones to exclude extraneous sounds. A button-press was held in each of the subject's hands: each illuminated one of two very small bulbs, 3 mm across, which were photographed along with the eye. These were painted over except for a tiny pinhole facing the camera, so that their illumination was undetectable by the subject. In this way a signal was put on the film whenever the subject made a response.

Material. All material consisted of continuous chains of three symbols (i.e. letters or digits) followed by a pause for a response if appropriate. The pause was equal in duration to that of a symbol, giving in effect consecutive blocks of four items. There were two variables imposed: speed and difficulty.

The rate of presentation was either fast or slow, at either 80 or 40 items per min. In this way either 8 or 16 film frames were to be taken for each block of material. The material was recorded on the beat of a metronome, the sound of which did not reach the tape.

Secondly, the intrinsic difficulty of the task was determined by the number of possible responses and number of decisions which were relevant. The easy tasks used number sequences, with a single response, that of pressing the right-held button-press if either of two conditions were satisfied. These were if the first and third digits were identical, or if the second digit was an odd number. The difficult tasks used letter sequences. Two possible responses were required when any of three conditions were satisfied. The right-held button was to be pressed either if the first letter came alphabetically later than the third, or if the second was a vowel. The left-held button was to be used when the second letter came alphabetically later than the third. There was never any incompatible or contradictory material, and half the blocks, randomly distributed, required no response at all.

The recorded order of presentation was counterbalanced to control for temporal effects, there being two sets of slow numbers, two of fast numbers, two of slow letters and two of fast letters. There were 12 blocks of three symbols and pause in each slow sequence, and 25 in each fast. This gave a recorded total of 48 slow and 100 fast blocks, occupying a period of nearly 10 min., 1568 frames, or 40 ft. of film, per subject.

Procedure. There were six subjects: three male and three female. All were adult members of the Psychology Department, aged between 20 and 34 yr. They were instructed to fixate the fixation point on the centre of the back-projection screen and not to shift fixation unless absolutely necessary. Displacement of reflected highlights on the film record enabled any such sequences to be discounted from analysis. This rarely occurred. On to the screen was projected, for the duration of each number or letter sequence, the appropriate summarized response instructions, should the subject forget what was required. However, considerable periods of prior practice were given with both types of material before any recording commenced and again before each fresh sequence, with the camera switched on at a specified point.

RESULTS

For all six subjects the average pupillary diameters for the four conditions, slow numbers, fast numbers, slow letters and fast letters were obtained. In every case there was seen an ascending order of magnitude, from slow to fast, and from numbers (easy) to letters (difficult), with slow letters always inducing higher dilations than fast numbers ($P < 0{\cdot}001$, $n = 23$, by one-tailed sign test). In only one instance was there a tie, between slow and fast letters, with a subject who said he was operating near his limit with the slow letters. The combined averages of all six subjects are presented in Fig. 1, where it is seen that the slopes are parallel, indicating that the effect of the two rates of presentation was the same for both orders of intrinsic difficulty.

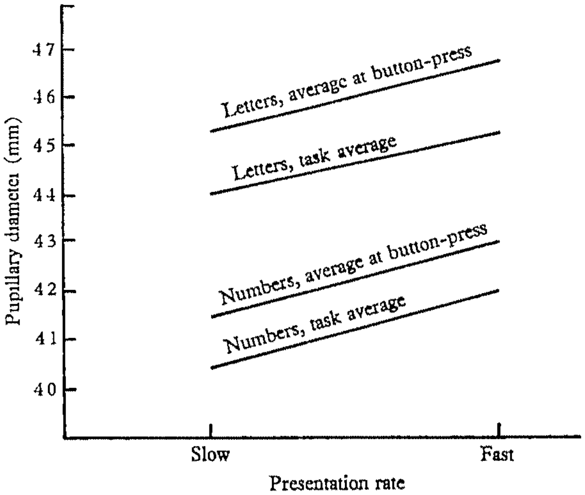

Fig. 1. Combined average pupillary diameters, from all six subjects, for the two rates of presentation and the two orders of processing load (letters–difficult, numbers–easy). The average dilations at the moment of button-press responding are also shown for the four conditions.

Also shown in Fig. 1 are the averages at the moment of button-press, as indicated by the signals on the film. There was an increase of about 0·1 mm in all instances. It is to be noted that though measurements were made to the nearest 0·1 mm, it was felt that the sample size justified averages to two places of decimals. When these results were considered block by block for each subject, in 41 instances the average button-press levels were greater than the overall block average by more than

0·05 mm, in four cases they were less, and in three cases there was no difference ($P < 0{\cdot}001$, one-tailed sign test).

In previous studies Bradshaw (1967) had noted that during the progress of a particular sequence there was a strong tendency for a gradual decline in pupillary dilation, due perhaps to declining arousal and increasing familiarity. In the 48 subject-block studied here, 33 instances supported this previously noted trend, 13 went against it, and in two cases the difference was less than the arbitrarily set criterion of 0·05 mm ($P = 0{\cdot}002$, one-tailed sign test). In fact, five of the 13 contrary instances occurred in a single one of the eight sequences, the second set of the fast letters, which provided a level of difficulty near the limit of the subject's capabilities. This was further borne out by the fact that the average decline was greatest for materials of intermediate difficulty, fast numbers and slow letters. Both provided figures of exactly 0·1 mm. The decline was smallest for the extremes, with an average of 0·05 mm. for the slow numbers, and a tendency towards a positive increase of 0·025 mm for the fast letters.

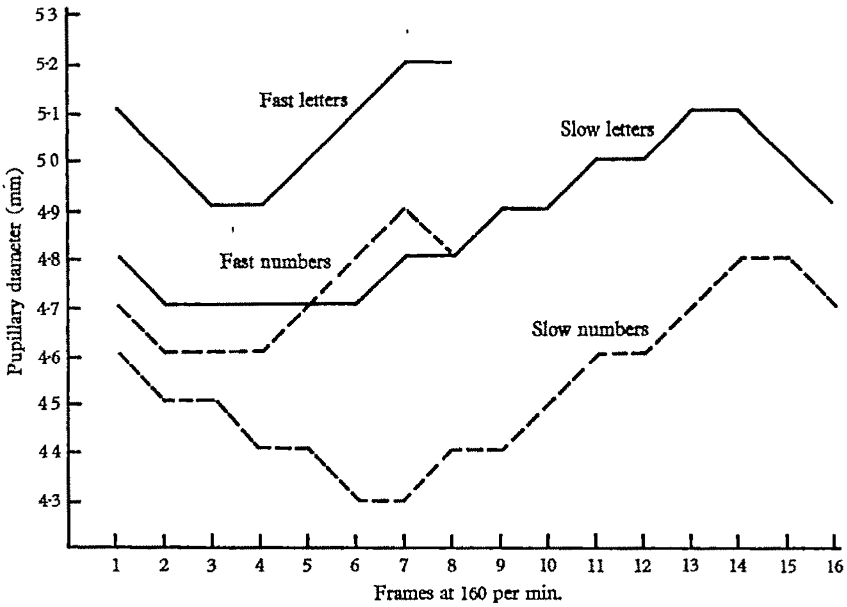

Fig. 2. The responses of a single subject, for the four experimental conditions. Averages were taken for successive frames during a block of three symbols and response pause. The response pause falls approximately at the 7th and 8th frames with the slow sequences, and during the 13th to 16th frames for the fast.

While measurements were being recorded, there developed increasing evidence of a regularly oscillating sine-wave effect, with two of the subjects; the peak in pupillary dilation occurred at or just before the response pause, irrespective of whether a button-press response was in fact made, and was followed by a rapid decline and subsequent build-up. The two subjects with whom this phenomenon was the most apparent were in fact the most competent at the tasks. With the other subjects, such a tendency was not so apparent, and frequently was obscured by premature or delayed responses. In Fig. 2 are shown the averaged responses of one of the two subjects. It was noted, however, that the pupillary measurements at or shortly before button-press were not significantly different from the corresponding moments in time when no responses were made. This could not so easily be verified with other subjects, who were more irregular in their rate and moment of responding. This meant that one could not point with certainty to the exact moment in the processing cycle when a response would have been made if called for.

DISCUSSION

The most important general finding is the confirmation of the hypothesis that cognitive load would determine pupillary dilations in a continuous processing task. Auditory material of the Wittenborn 'attention' type proved eminently suitable for controlling such load variables as rate of presentation, number of response alternatives and number of transforms or comparisons. Indeed, an almost completely linear relation is seen along the experimental continuum 'slow easy – fast easy – slow difficult – fast difficult'. The rate of presentation is doubled from 'slow' to 'fast', and with 'difficult' the number of comparisons is increased from two to three, and the number of relevant responses from one to two. If the situation is viewed in terms of a decision tree (Hunt *et al.*, 1966), in the 'easy' situation there are only two nodes, as seen in Fig. 3. However, with the difficult tasks a 'tree' of six nodes would operate, at least initially (Fig. 4). The number of nodes could be reduced to not less than four by recoding the response decisions, with increase of practice or familiarity.

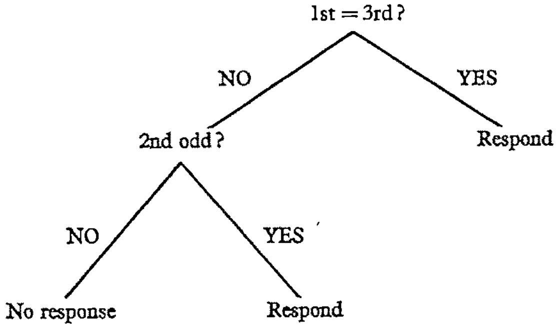

FIG. 3. Decision tree likely to operate when processing the easy material (number sequences). Two comparisons and one response are relevant.

The occurrence of further dilation at the moment of response, with the button-press levels rising about 0·1 mm higher than the overall averages, confirms previous findings (Bradshaw, 1967). Here it was found that there tended to be a peak at solution, and one at response, verbal or otherwise, even when the response and the solution were separated in time. It remains to be seen as to what extent motor, processing or emotive factors determine this.

However, in connexion with this there is the further phenomenon to be explained, of the sine-wave effect noted in two of the subjects. It could be the case that here the button-press response peaks necessarily coincide in time with that period in the cycle when dilation is near maximal. Certainly there were no significant differences apparent in pupillary levels near the ends of the cycles when they were divided into response and no-response groups. However, such an analysis was only feasible in those two subjects who evidenced regularly timed responses, together with strong cyclic oscillation. A further piece of relevant evidence is the finding of Kahneman & Beatty (1966) that, in a short-term memory task, pupillary dilation tends to accompany cognitive input, and constriction to occur during output. On the other hand, both phenomena might be merely special cases of the usual effect (Bradshaw, 1967), whereby rapid constriction follows solution and/or response.

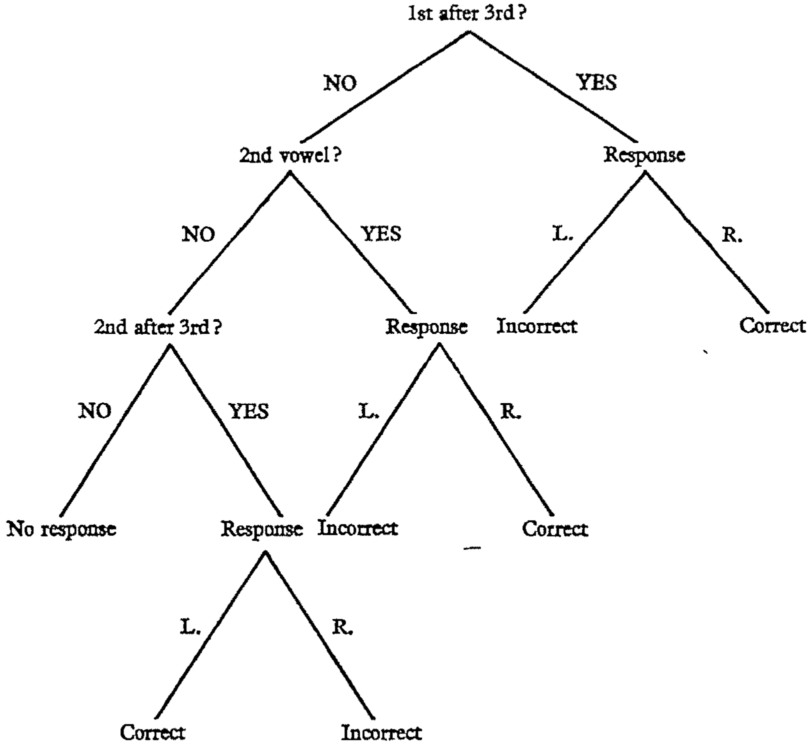

Fig. 4. Decision tree likely to operate when processing the difficult material (letter sequences). Three comparisons and two possible responses are relevant.

Finally, it is noted that a tendency to a gradual decline in dilation continues to occur during exposure to a block of uniform material. That this accompanies or even is determined by declining arousal is borne out by the fact that it is least evident with the easiest material which has been the subject of considerable practice, and also with the most difficult material, which nevertheless continues to exercise the limits of the subject's processing capabilities.

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SUPPRESSION OF THE UCR IN HUMAN EYELID CONDITIONING AS A FUNCTION OF CS INTENSITY

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Subjects were run in an eyelid conditioning experiment with CS intensity as the independent variable. Strength of conditioning was assessed by measures of UCR suppression and percentage CRs. It was found that increases in CS intensity produced increases in both percentage CRs and UCR suppression. This supported the view that UCR suppression was an associative process. In that, across groups, increases in percentage CRs were accompanied by increases in UCR suppression and, within groups, the two measures were uncorrelated, it was suggested that inhibition of delay was not the underlying mechanism of UCR suppression.

The primary purpose of this experiment was to determine the extent to which suppression of the eyelid UCR is under the control of CS intensity.

Kimble & Ost (1961) and Kimmel (1966) have presented data which indicated that UCR suppression, i.e. the reduced amplitude of the UCR on conditioning trials relative to UCR amplitude on UCS-alone presentations, was related to strength of conditioning. Kimble & Ost, in human eyelid conditioning, found that the extent of UCR suppression tended to be related to the parameter of ISI in a conventional manner, and Kimmel, in human GSR conditioning, reported similar findings with regard to the parameter of number of reinforced trials. (Kimble & Ost (1961), Nicholls & Kimble (1964) and Kimmel (1966) have reported the systematic decline in UCR amplitude during conditioning trials. Kimble & Ost did not relate this decline to their ISI parameter. Nicholls & Kimble, however, showed that the decline in UCR amplitude during conditioning trials was greater under 'inhibitory' instructions than under 'facilitatory' instructions.)

Thus far, CS intensity effects on UCR suppression have not been investigated. If the relation between strength of conditioning and UCR suppression holds under a variety of parameters, then it is predicted that increases in CS intensity will produce increases in UCR suppression.

METHOD

Subjects and procedure. The subjects were 72 undergraduate students from introductory psychology courses at the University of Cincinnati. Eighteen subjects (13 male and five female) were randomly assigned to each of four groups, with CS intensity as the independent variable. All subjects received standard instructions (see Fishbein, 1967).

Two subjects were run simultaneously in separate IAC Model 402A sound-treated rooms. The pick-up and recording apparatus primarily consisted of a Waltke head-set with attached Giannini potentiometer, and an Offner 'Dynograph'.

The subjects wore Model D 30 Grason-Stadler earphones through which, approximately 65 db above threshold (79 db SPL), white noise (0–20 kc) was continuously present. The four CS (1 kc) intensities were approximately 0 db (65 db SPL), 7 db (72 db SPL), 19 db (84 db SPL), and 32 db (97 db SPL) above threshold. The threshold measurements were obtained by the Method of Limits with a 0·5 sec. signal on an independent group of subjects. (These SPL readings were obtained by converting voltage readings across the earphones to SPL according to the manufacturer's specifications.) The UCS was a 0·05 sec., 1·5 psi corneal air puff. The CS duration (and ISI) was 0·3 sec. Sixty trials of acquisition were run, under 100 per cent reinforcement, followed, without changing the pattern of presentation, by five trials in which the UCS alone was presented. The inter-trial interval for all 65 trials was either 7·5 sec., 15·0 sec. or 22·5 sec., with a mean of 15 sec.

Response measures. A CR was scored if a 2 mm or greater eyelid closure occurred in the interval 0·16–0·33 sec. after CS onset. A procedure for scoring UCR amplitude, similar to that of Kimble & Ost (1961), was adopted. A UCR was defined as the first response which commenced in the interval 0·03–0·2 sec. after UCS onset. Its amplitude was defined as the maximum perpendicular distance from the base-line that it travelled. Amplitude measurements were made on the last five conditioning trials in which no CR occurred, and on those UCS-alone trials in which spontaneous blinks did not overlap UCS onset. Two mean UCR amplitudes were obtained for each subject: one based on conditioning trial amplitudes, and the other based on UCS-alone trial amplitudes.

RESULTS

Table 1 presents a summary of the experimental results. The group designations in Table 1 are in terms of the CS intensity employed. For example, group 32 was run with a 32 db above threshold CS intensity. The first column in Table 1 presents mean percentage uncorrected CRs over trials 41–60 for each experimental group. As can be seen, decreasing CS intensity led to fewer CRs, with the sharpest drop occurring as CS intensity decreased from 19 db to 7 db. Statistical analysis bears out this observation ($F = 10{\cdot}6$; 3, 68 d.f.; $P < 0{\cdot}01$).

Table 1. *Percentage CRs, amplitude UCRs (in mm), and two measures of UCR suppression as a function of CS intensity*

Group	% CRs: trials 41–60 (1)	Amplitude UCRs: conditioning trials (A) (2)	Amplitude UCRs: UCS-alone trials (B) (3)	B − A *r* B/A (4)	B − A (5)	B/A (6)	B − A *r* CRs (7)	B/A *r* CRs (8)
32	55	13·2	19·3	0·91	6·1	1·43	−0·17	−0·27
19	52	15·4	20·8	0·67	4·4	1·09	−0·32	0·03
7	19	14·5	15·8	0·88	1·3	0·94	0·31	0·31
0	6	15·7	13·7	0·50	−2·0	0·48	0·21	0·44

The second column (A) of Table 1 presents mean amplitude of UCRs on the last five conditioning trials in which CRs failed to occur, and the third column (B), mean amplitude of UCRs on the five UCS-alone trials, respectively. Although UCRs on conditioning trials tend to be smaller for group 32 than the remaining groups, and smaller on UCS-alone trials for group 0 than the remaining groups, these differences were not significant (column A: $F = 0{\cdot}4$; 3, 68 d.f.; $P > 0{\cdot}05$; column B: $F = 2{\cdot}4$; 3, 68 d.f.; $P > 0{\cdot}05$).

The primary focus of this paper is on the relative amount of UCR suppression (over and above UCS adaptation) produced by different CS intensities. The extent of this suppression can be inferred (following Kimble & Ost, 1961; Kimmel, 1966) from comparisons of UCR amplitude on conditioning trials with UCR amplitude on subsequent UCS-alone trials. We know of no theoretical reason for assuming that the appropriate comparison is of the differences as opposed to the ratios of these amplitudes. Hence, both comparisons were made. The fourth column of Table 1 indicates that these two measures are highly correlated.

The fifth and sixth columns of Table 1 present amplitude of UCRs on UCS-alone trials minus UCR amplitude on conditioning trials (B–A), and the ratio of UCR amplitude on UCS-alone trials to UCR amplitude on conditioning trials (B/A), respectively, as a function of CS intensity. As can be seen, with both measures, the extent of UCR suppression was positively related to CS intensity, in agreement with the experimental prediction. Statistical analyses bear out this observation (for B–A: $F = 6{\cdot}3$; 3, 68 d.f.; $P < 0{\cdot}01$; for B/A: $F = 3{\cdot}8$; 3, 68 d.f.; $P < 0{\cdot}05$).

The seventh and eighth columns of Table 1 present the product-moment correlations for each group between percentage CRs on trials 41–60 and both B–A and B/A. As can be seen, all the correlations are small, and none is significantly different from 0 (all P values are greater than 0·05).

DISCUSSION

The major findings of this experiment were: the amount of both UCR suppression and percentage CRs was positively related to CS intensity and, within each group, percentage CRs and UCR suppression were not significantly correlated.

The data of the present study, along with those reported by Kimble & Ost (1961) and Kimmel (1966), clearly indicate that UCR suppression does reflect strength of conditioning. However, it is not clear that the process underlying this phenomenon in eyelid conditioning is ***inhibition of delay*** as Kimble & Ost (1961) and Kimmel (1966) have suggested.

Reconsider Table 1. Note that percentage CRs and UCR suppression are not significantly correlated within any of the groups. This indicates that subjects who showed the most CR suppression did not show the most UCR suppression. Kimble & Ost reported similar findings. If inhibition of delay were producing UCR suppression, then subjects who showed the most UCR suppression should have shown the most CR suppression. Further, groups which showed the *greatest* UCR suppression showed the *least* CR suppression. Kimble & Ost reported similar findings for their ISI data. If UCR suppression were produced by inhibition of delay, the opposite should have occurred. These two observations seriously question the validity of the inhibition of delay hypothesis. That inhibition in eyelid conditioning can have the effects demanded by inhibition of delay has been demonstrated by Nicholls & Kimble (1964). In their study, subjects who received inhibitory instructions showed greater CR and UCR suppression than did subjects who received standard instructions.

Some possible resolutions to these apparent inconsistencies between the inhibition delay of hypothesis and the above data involve the following potential revisions of the hypothesis: (1) inhibition and excitation do not operate simultaneously in eyelid conditioning, and subjects and groups who have developed the most inhibition also have developed the most excitation; (2) inhibition in eyelid conditioning only affects the UCR, and excitation only the CR; (3) there is more than one kind of inhibition; and (4) UCR suppression in eyelid conditioning may be an excitatory phenomenon. The validity of any of these possibilities cannot be assessed, however, until we have a more precise statement of the inhibition of delay hypothesis and the subsequent experimental tests.

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DISINHIBITION AND REMINISCENCE

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The present experiment investigated the effect of a visual disinhibitor (instead of an auditory stimulus as used by Rachman, 1962) on reminiscence in a human pursuit-rotor task. Four groups of 10 subjects each took part. A control group was presented with no light flash during practice. Group E 1 was presented with the light flash early in a 5 min. practice period, group E 2 in the middle and group E 3 late in the practice period. The results indicate an adverse effect of disinhibitor on reminiscence, significantly marked when presented very late in the course of practice.

Rachman (1962) found, in his twin experiment, support for the hypothesis that the introduction of an alien or extraneous auditory stimulus late in the practice period will produce disinhibition and a decreased reminiscence effect. He interpreted the results in terms of Pavlov's (1927) description of inhibition and Eysenck's (1965) account of the development and dissipation of reactive inhibition. The present study was designed to demonstrate that the deliberate introduction of an alien stimulus, a light flash, can interfere with the normal course of the inhibitory process and produce a decreased reminiscence effect.

METHOD

A Lafayette pursuit rotor (Rachman, 1962; Mohan, 1966) with a 10 in. diameter disk, rotating in a clockwise direction at 60 r.p.m., and with a ¾ in. diameter contact point, was used in this experiment. The contact point or target was set with its centre 3¼ in. from the centre of the turntable and flush with the surface. The stylus (total weight 2 oz.) consisted of a circular plastic handle (4½ in. long) with a guard set 1 in. from the end of the handle. The extension rod (6 in. long, $\frac{1}{10}$ in. diameter) was hinged so that only its weight rested on the turntable, and it had an 85° bend 1 in. from its end. When steady contact was made between the stylus and the target, the electric circuit was completed.

Two electric timers, connected to the rotor, were calibrated to read the time on target in tenths of a second. They were wired in such a way that they alternated in recording the time on target every 10 sec. (the length of each trial). Calibration and zero adjustments were made prior to the testing of each subject and the entire apparatus was switched on ½ hr. before testing commenced and kept running during the rest period. The stylus point and target were cleaned with a smooth cloth after every 15 min. of operation.

The rotor was set on a table and the target was 30 in. off the floor. The recording clocks and scoring sheets were positioned so that the subject was unable to see them.

Four groups of 10 female subjects each were used. Each subject was given 5 min. of continuous practice (30 trials) followed by a 10 min. rest period. The subject was then retested for a further 1 min. period. In the case of the control group, no light was presented during practice. In the case of the three experimental groups (E 1, E 2, E 3) the light was presented for 2 sec. after 90 sec., 190 sec. and 290 sec. of practice respectively. The light was timed with a stopwatch and was presented by keeping the telegraphic key connecting a 100 W bulb pressed for 2 sec. The bulb was located on the table at a distance of 1 ft. from the pursuit rotor, and was directly visible to the subject.

Table 1. *Means and S.D.s of age and personality scores*

	Age		Neuroticism		Extraversion	
Group	Mean	S.D.	Mean	S.D.	Mean	S.D.
Control	20·1	1·4	16·8	6·2	25·4	8·7
E 1	20·4	1·8	15·9	5·9	25·8	7·4
E 2	19·9	2·1	16·1	6·3	26·1	7·8
E 3	20·3	1·7	16·5	5·7	25·5	7·9

Table 2. *Means and S.D.s of post-rest increments (sec.)*

	Group			
	Control	E 1	E 2	E 3
Mean	0·78	0·50	0·44	0·28
S.D.	0·23	0·24	0·18	0·14

Table 3. *Analysis of variance of reminiscence scores*

Source of variance	S.S.	D.F.	M.S.	*F*	*P*
Between groups	130·4	3	43·5	16·7	0·01
Within groups	93·6	36	2·6		
Total	224·0	39			

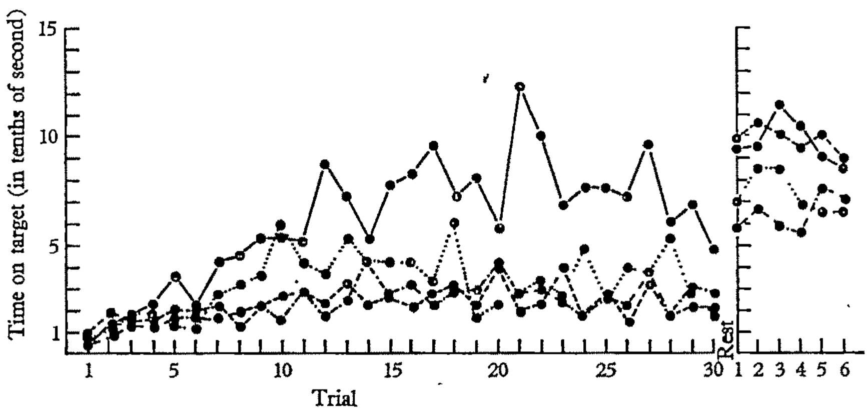

Fig. 1. Performance on the pursuit-rotor task. – – – – –, control group; · · · · ·, E1 group; —, E2 group; ·—·—, E3 group.

The subject was shown the apparatus and instructed to attempt to keep the stylus on the target continuously, and throughout the testing period, and to chase it if she lost contact. After a demonstration by the experimenter, the subject was given a brief trial on the apparatus to ensure that she comprehended the nature of the task. The subject was then instructed to proceed with the task until told to stop.

Table 1 presents the means and S.D.s of scores on E and N (Eysenck, 1959) and means and S.D.s of age of the subjects in all four groups. The subjects were randomly assigned to the control or experimental groups. Each subject was tested individually.

RESULTS

The reminiscence score for each subject was obtained by subtracting the score on the last pre-rest 10 sec. trial, in terms of time on target, from the score obtained from the first post-rest 10 sec. trial.

Table 2 shows the means and S.D.s of the reminiscence scores. Table 3 presents the analysis of variance, which shows a highly significant F ratio of 16·7 (1 per cent level).

Fig. 1 shows the pursuit-rotor performance of all four groups of subjects.

DISCUSSION

The analysis of variance as well as the curves sustain the hypothesis and support the findings of Rachman (1962). The introduction of an alien stimulus, a visual disinhibitor, late in practice produced a significant effect on reminiscence in the pursuit-rotor task (F ratio being significant at the 0·01 level). The results are explicable in terms of reactive inhibition, conditioned inhibition and consolidation (Eysenck, 1965; Rachman & Grassi, 1965).

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SEASON OF BIRTH, INTELLIGENCE AND PERSONALITY

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Intelligence and the personality dimensions of extraversion–introversion and neuroticism were studied as a function of season of birth, the latter being divided into birth during Winter–Spring (W–S) (November–April inclusive) and Summer–Autumn (S–A) (May–October inclusive). Two hundred and thirty adolescent males, homogeneous with respect to age, occupation and socio-economic background, were tested under controlled conditions. No differences in intelligence and personality, as measured, were found between W–S and S–A births. Little optimism was expressed for the possibility of finding such season-of-birth effects among other than severely intellectually subnormal subjects.

In the recent psychological literature, the beneficial effect on intelligence of Summer–Autumn birth as compared to Winter–Spring birth often appears to be a 'now you see it, now you don't' phenomenon. Orme (1962, 1963) has been the most successful in demonstrating the beneficial effects of Summer–Autumn birth, in line with earlier work summarized by Fitt (1941), whereas Davies (1964) could find no such effects and Craddick (1966) reported the contrary result of higher IQ in subjects born during the *Winter–Spring* as compared to Summer–Autumn months. In all the studies cited, Winter–Spring (W–S) was defined as including November–April inclusive, whereas Summer–Autumn (S–A) included May–October.

Orme's research was based on intellectually subnormal adults. Davies used intellectually normal male adults. She also measured introversion and emotionality (neuroticism), but found no relation between these personality variables and season of birth. Craddick (1966) reported two studies, one of children and the other of adults, with both studies employing samples having IQs above 90. In the latter study, however, the mean age of the W–S subjects was significantly greater than that of the S–A subjects! This factor alone could have accounted for the higher mean IQ of the W–S group in that study. The month of testing also was not controlled, though Lodge (1938) has shown that the month of testing can influence intelligence test scores.

The present study was undertaken to assess the relation of season of birth to intelligence, extraversion–introversion and neuroticism under conditions in which controls over age, sex, occupation, and time and conditions of test administration were exercised, and in which no restrictions were placed on the range of scores of the intelligence and personality variables investigated.

The subjects in the present study were 230 (British) male trade apprentices (age range 15–17 yr.; mean = 16·61). Most of the subjects were drawn from a highly similar socio-economic background, and nearly all came from the south of England. All were studying for one trade out of six for which training was available at the training school.

The measure of intelligence employed was Group Test 33 (GT 33) of the National Institute of Industrial Psychology, which is a verbal test containing five subtests, of which only the first four were used in the present study. Standardized administration allows 5 min. to complete each of the first three subtests, and 10 min. for the fourth test.

The personality measure was the Eysenck Personality Inventory (EPI) which was scored for extraversion–introversion (EPI–E) and neuroticism (EPI–N).

The subjects were all tested during two days (November) in groups of 35–45 with the same test administrator and assistants for all groups.

The distribution of GT 33 scores was plotted and found to be quite normal, though with a very slight positive skew. The GT 33 mean was 103·60, range 46–152, SD = 16·49. Of the 230 subjects tested, 119 were S–A births (mean age = 16·64 yr.) and 111 were W–S births (mean age = 16·57 yr.). These mean ages were not significantly different by *t* test. The mean GT 33 score of the S–A subjects was 103·52 (SD = 16·31) while that of the W–S subjects was 103·69 (SD = 16·74). The *t* test in this analysis was also not significant.

Table 1. *Frequency of S–A and W–S births at three levels of intelligence*

Season of birth	Low intelligence	Middle intelligence	High intelligence	Both
S–A	34	48	37	119
W–S	42	30	39	111
	76	78	76	230

Recent work demonstrating significant mean differences in intelligence between S–A and W–S subjects (Orme, 1962, 1963) has been based on *intellectually subnormal persons*. It is possible with the present sample to examine more closely the relationship of extreme intelligence scores to season of birth by dividing the total GT 33 score distribution into thirds (low, middle and high intelligence) and comparing the distributions of S–A and W–S scores by χ^2. Accordingly, the total GT 33 distribution was divided into a high-intelligence group ($n = 76$, GT 33 scores 111–152), a low-intelligence group ($n = 76$, GT 33 scores 46–97) and a middle intelligence group ($n = 78$, GT 33 scores 98–110). The contingency table for these data is found in Table 1. Analysis of the data in Table 1 yielded a χ^2 of 4·776 (2 d.f.; $P > 0{\cdot}05$).

Turning to the personality data, the mean EPI–E score of the S–A group was 14·07 (SD = 3·83), while that of the W–S group was 13·86 (SD = 3·78), but the mean difference here was not significant by the *t* test. The mean EPI–N score of the S–A group was 9·83 (SD = 4·10), while that of the W–S group was 9·90 (SD = 3·96); the difference between these means likewise did not achieve significance.

The present study has clearly failed to demonstrate a difference in level of intelligence associated with S–A or W–S birth. The results confirm those of Davies (1964), and are supported by the work of Ojha *et al.* (1966) and Crookes (1963). Orme (1962, 1963) remains the major recent source of support for the significant relationship of season of birth to intelligence, and his sample, as noted above, was an intellectually subnormal one. The weight of contemporary evidence suggests that if season of birth is related to intelligence, this relationship most likely holds *only among severely subnormal persons*.

Where the personality dimensions of extraversion–introversion and neuroticism are concerned, the lack of a significant difference between the S–A and W–S groups on both dimensions confirms the similar findings of Davies (1964).

The data for this study were collected while the author was at the Institute of Psychiatry, University of London.

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DIMENSIONAL DOMINANCE IN DISCRIMINATION LEARNING: A STUDY OF SEVERELY SUBNORMAL AND NORMAL SUBJECTS

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Non-mongols, mongols and normals were compared on matching tasks in which solution depended on a dimensional response to either shape or colour. Original learning with shape relevant and colour irrelevant, or the converse, was followed by reversal learning in which these dimensions were switched. Non-mongols were found to be slightly shape-dominant, compared to normals, while mongols were strongly colour-dominant. Verbal data suggested that the ability to formulate concepts could be related to task difficulty and/or availability of specific labels for stimuli. It was found that an increased criterion stringency in original learning facilitated reversal performance in the more difficult task and in both subnormal groups. The implications of this finding were discussed, with reference to the qualitative nature of the learning process, and its relationship to task complexity and difficulty level.

Recent discrimination learning research concerned with the validation of two-stage learning theories, such as those of Sutherland (1959) or Zeaman & House (1963), has shown a growing recognition of the role which dimensional dominance (also termed cue dominance or stimulus potency) can play in the mechanics of transfer processes. In particular, it has been necessary to explain in terms of cue dominance some of the anomalies which have been observed in connexion with the overlearning reversal effect (ORE). It is recognized (Paul, 1965; Mackintosh, 1965) that in a classical reversals experiment involving only one dimension or cue, the relatively low potency of the relevant cue is a prerequisite condition for the appearance of ORE. Where two relevant cues are present and competing (Sutherland & Holgate, 1966) the effect of overlearning is to strengthen performance in terms of the weaker cue. When a dimension is sufficiently predominant, the organism does not have to learn to attend to it, and only has to form response attachments to the stimuli along it; in short, a two-stage theory contracts to a one-stage theory.

References in the literature to factors underlying dimensional dominance are diverse, though not mutually exclusive in their implications. In the animal field it is recognized that both intra-species and inter-species differences exist. The latter have been attributed to ecological factors (Sutherland, 1964), the phyletic level of the organism (Warren, 1953; Draper, 1965) and its sensory equipment. It has also been suggested (Draper, 1965) that stimuli along dimensions with an underlying physical continuum are relatively easy to discriminate and that such dimensions tend to be dominant; colour would thus be dominant over shape. This notion would be in accordance with Hebb's theories of visual perception and also with the theory that progression from colour to shape dominance is part of the developmental process in human subjects. The latter has received experimental support from a series of studies, including those of Brian & Goodenough (1929), Corah (1964) and Suchman & Trabasso (1966); the findings of these studies indicate a consistent colour-to-form shift, young children under 5 yr. (like animals) tending to be colour-dominant, with older children and adults tending to shape-dominance.

Comparative research in the human field has been concerned with the possibility of a real qualitative difference, such as that postulated by Bryant (1965), between the learning and transfer processes of severely subnormal subjects and their MA matched normal peers. A satisfactory resolution of this issue depends on the control of those variables, including dimensional dominance, which might affect these processes. The matching procedure in such studies frequently results in a considerable age gap between normals and subnormals; this gap, moreover, often includes what is thought to be a critical transition period (around 6 yr.) in the colour-to-form shift. In this case, the assumption of equivalence in dominance between the populations concerned would seem to be a somewhat questionable procedure. House & Zeaman (1963), investigating dimensional dominance in a subnormal population, have found some increase in shape preference with increase in MA. It would also seem reasonable, however, to expect adult subnormals to be relatively shape-dominant compared to MA matched normals by virtue of their greater chronological age. This problem is further complicated by the occurrence of pathological factors in a subnormal population which could affect both perceptual processes and the colour-to-form shift. Examples of such are the verbal deficit, recognized to be a feature of subnormals, and colour-blindness, which was found by O'Connor (1957) to have a relatively high incidence in a subnormal population. Some perceptual abilities have been found to differentiate within the subnormal population itself; mongols, for instance, have been found to be markedly poor compared to MA matched normals in the stereognostic recognition of shape, whereas their non-mongol peers are virtually unimpaired (i.e. equal to CA matched normals) in this respect (O'Connor & Hermelin, 1963).

The aim of the present experiment was to assess colour–shape dominance in three groups of subjects: normal children, and mongols and non-mongols of severely subnormal range. A matching task was used in which the subject had to learn consistently to match a sample card to one of several choice cards in terms either of colour or shape, whichever was relevant. As the colour and shape of the sample varied from trial to trial, all stimuli along both colour and shape dimensions were equally associated with reward. The effect of stimulus factors was thus minimized and attainment of criterion depended on the matching response, which was dimensional in nature. It was assumed that, in such a task, learning rate would vary with the degree of dominance of the relevant dimension. Two tasks were presented to each subject, with approximately an 8-week interval intervening. Stimulus values for the dimensions of colour and shape were different in the two tasks and the second task was designed to be of greater difficulty than the first. It was thus possible to obtain data on the colour–shape response from two quite different sets of material.

The transfer paradigm was the same in both tasks and for all subjects and consisted of two stages: (1) original learning, to a criterion of six or 12 successive correct trials, with colour relevant and shape irrelevant or the converse; and (2) reversal learning, to a criterion of six successive correct trials, in which the relevant and irrelevant dimensions of stage (1) were switched. Thus only negative transfer of the dimensional response was arranged and transfer of stimulus specific responses was not possible, as learning of these responses was absent from the task itself.

The rationale behind most discrimination learning experiments using dimensional shift is that learning is controlled by the acquisition of a relevant dimensional response. This has proved on the whole to be a workable hypothesis and, in the transfer paradigm used here, would predict negative transfer between original and reversal stages of learning. In other experimental situations, however, such as that used by Bryant (1965), transfer effects were explained in terms of a strategy to avoid the irrelevant dimension. In this present experiment, criterion stringency was varied in an attempt to determine more precisely the limitations of the relevant dimensional response as an explanatory construct in the later stages of learning, and the possible role of the irrelevant dimension. As long as learning was controlled by a response to the relevant dimension, additional trials would have either no effect on reversal or an impeding effect, in the event of response strength falling short of asymptote. A facilitating effect, however, would require that something other than a relevant dimensional response was acquired during the additional criterion trials.

Verbal data, in this experiment, were obtained at the conclusion of learning in each task, and were incidental to the main experimental design. It was thought, however, that they would provide useful information on the availability of verbal mediators appropriate to a dimensional response.

METHOD

Subjects. Three groups of 28 subjects each took part in the experiment. Mongol and non-mongol groups were institutionalized and of severely subnormal grade; the normal group was drawn from local infants' schools. Groups were matched on a criterion score, based on a modified version of Coloured Progressive Matrices (Williams & Wilcock, 1966). Where appropriate, subjects were also matched on CA. The mean criterion score of the non-mongol group was 12·29 (S.D. = 3·09): of the mongol group, 12·50 (S.D. = 3·60): of the normal group, 12·89 (S.D. = 3·01). The mean CA of the non-mongol group was 17·02 (S.D. = 3·98); of the mongol group, 17·90 (S.D. = 2·54); of the normal group, 5·29 (S.D. = 0·17). The MA of the severely subnormal groups was approximately equal to the CA of the normal group.

Tasks. Two tasks, A and B, were given to each subject, task B approximately 8 weeks after task A. Each task consisted of (1) original learning, with colour relevant and shape irrelevant or the converse, and (2) reversal learning, in which the dimensions of original learning were switched, relevant to irrelevant, and vice versa. For any one subject, relevance of dimension was constant for the two tasks. In both tasks, both original and reversal learning involved choosing from three or four choice cards, a match for the sample card, which varied in shape and colour from trial to trial. In any one trial it was possible to make this match in terms of colour or shape, but not both, and the subject had to learn to match consistently in terms of the relevant dimension. In every trial, only the card which matched the sample in terms of the relevant dimension was rewarded. As the colour and shape of the sample varied over successive trials, so did that of the rewarded card; in this way, any preference for a particular shape or colour was cancelled out. The position of correct shape or colour responses was randomly varied and, over a series of trials, occurred with equal frequency in each position. Both original and reversal learning started with trial 1 of the series (see Figs. 1 and 2).

The stimuli were coloured paper shapes mounted on black cards, 3 × 4 in. In task A, three choice cards, together with a sample card, were presented on each trial. Colours were red (R), yellow (Y) and green (G); shapes were a square, circle and triangle. To randomize both the position of rewarded cards, and the stimulus values themselves, a recurring series of nine trials was used, the first four of which are shown in Fig. 1. Material was prearranged on nine trays with the choice cards side by side and the sample card on top. On each tray, the reward (a brown

'Smartie') was placed in a groove, under the correct card. At the conclusion of original learning, rewards were changed to conform to the new relevant dimension of reversal learning.

In task B, four choice cards were presented on each trial with the sample card. Colours were pink (P), mauve (M), blue (B) and turquoise (T); shapes were a diamond, oblong, pentagon and trapezium. A recurring series of four trials was used as shown in Fig. 2, and cards were prearranged with rewards in place, on four trays. At the conclusion of original learning, rewards were left in place and a new set of samples substituted; thus the same choice cards were rewarded, but represented a colour match where they had previously represented a shape match and vice versa. With the smaller number of trials and sample cards, this was found to be a more convenient method of adjusting material for reversal than that used in task A.

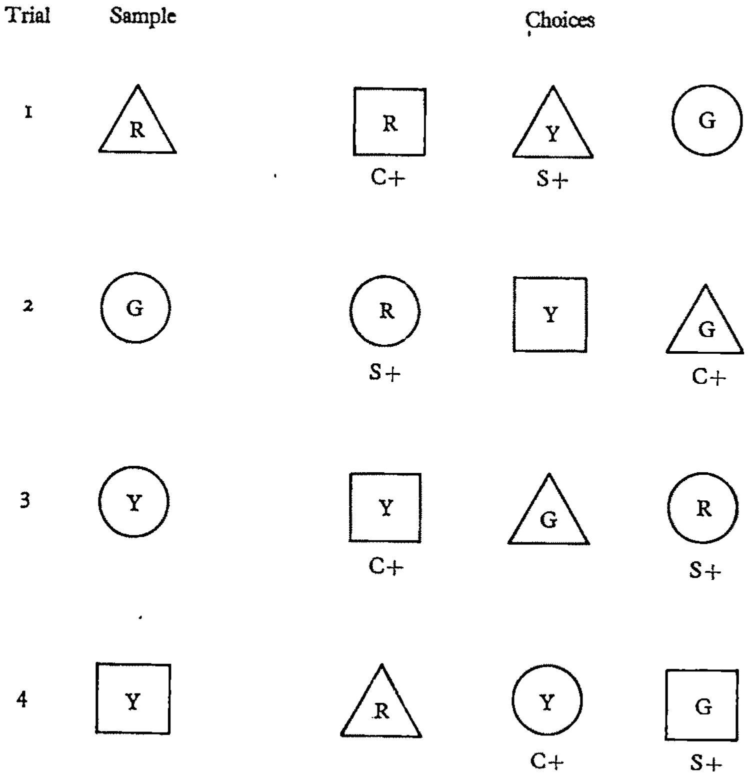

Fig. 1. Task A: arrangement of stimuli in the first four trials. Colours are indicated by the capitals R (red), Y (yellow) and G (green). C+ indicates rewarded cards in colour relevant; S+ in shape relevant learning. All stimuli were mounted on black cards, 4 × 3 in.

It was intended that task B should be more difficult than task A. Task B colours were closer on the dimensional continuum than those of task A; task B shapes were more complex than task A shapes. In addition, task B involved four examples of each dimension and four alternative choices on each trial, instead of three, as in task A.

Procedure. The subject was seated in front of the prearranged material. A large screen ensured that the contents of only one tray at a time were visible to him. On each trial the sample card was placed centrally, in front of the tray, and the subject's attention was drawn to it. He was told to find one like it from the choice cards on the tray and that, if correct, he would find a 'Smartie' underneath. He was encouraged to pick up the card of his choice. If correct, he found the hidden 'Smartie', placed it in a container and was praised. He watched another 'Smartie' being placed under the correct card. If incorrect, the subject was shown the correct card and the reward, but was not allowed to take it. Original learning proceeded to a criterion of six or 12 successive correct trials. This was followed, after a brief pause for rearrangement of either rewards or samples, by reversal learning, to a criterion of six successive correct trials. No comment was made on reversal and procedure was as for original learning.

Responses were recorded as either colour, shape or incorrect. At the conclusion of reversal learning in both tasks, the subject's ability to verbalize both colour and shape concepts was tested by the question, 'How are these two cards alike?', using a sample and appropriate choice card, or if necessary, 'They are both the same...'. Failure to elicit the required concept words (colour, shape or acceptable alternatives) was followed by further questioning, using a different sample. The subject was then asked to name all colours and all shapes.

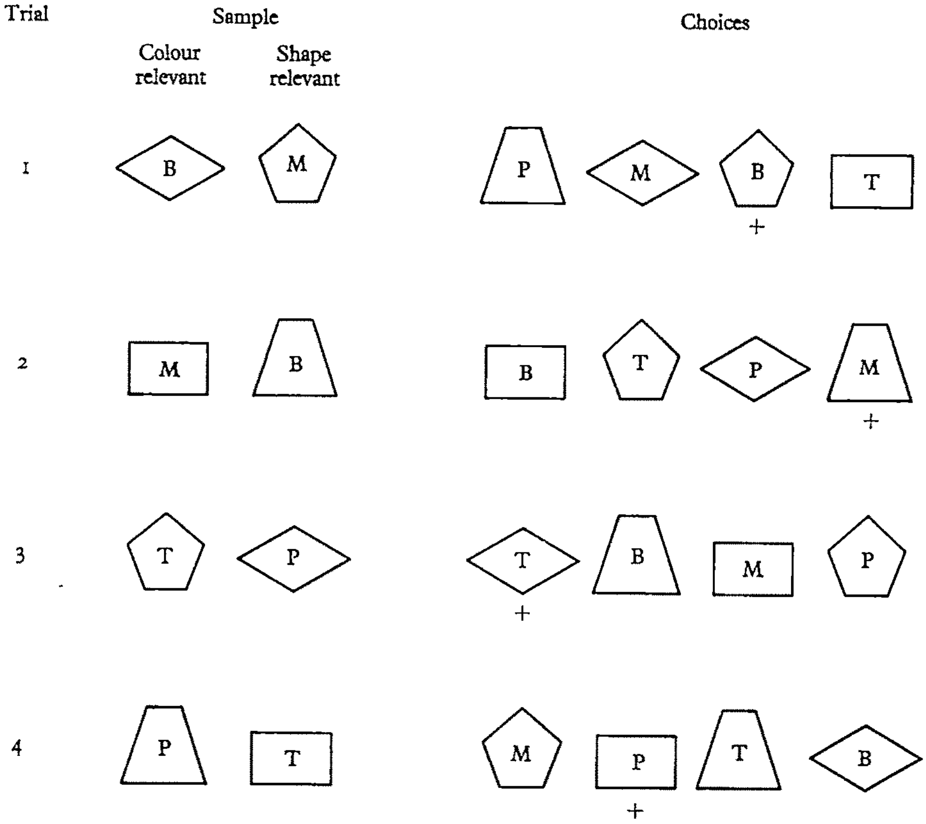

Fig. 2. Task B: arrangement of stimuli in the first four trials. Colours are indicated by the capitals B (blue), T (turquoise), P (pink) and M (mauve). + indicates the rewarded cards in both colour and shape relevant learning. All stimuli were mounted on black cards, 4 × 3 in.

Design. The same 3 × 2 × 2 factorial design was used for each task. The controlled variables were groups (the three groups of subjects), conditions (colour or shape relevant) and criterion (six or 12 successive correct trials in original learning). Thus each subject group of 28 was subdivided into four subgroups of seven subjects each, corresponding to the four combinations of rewarded dimension and criterion stringency.

RESULTS

General transfer effects

In task A, original learning rate was more rapid (though not significantly so) in the normal group; non-mongols and mongols, however, showed some improvement from task A to B, so that in the second more difficult task their original learning rate approximated to that of the normals. The first trial of reversal learning was unrewarded, except for three normal subjects in task A and two in B, who presumably took the slight pause necessary for rearrangement of material as a cue to reverse. Discounting this trial, reversal resulted in zero transfer for the normal group, in both tasks, and negative transfer for the non-mongol and mongol groups; the latter was more marked in task B. Thus there was no evidence, in any group, that learning to reverse had occurred. (See Fig. 3.)

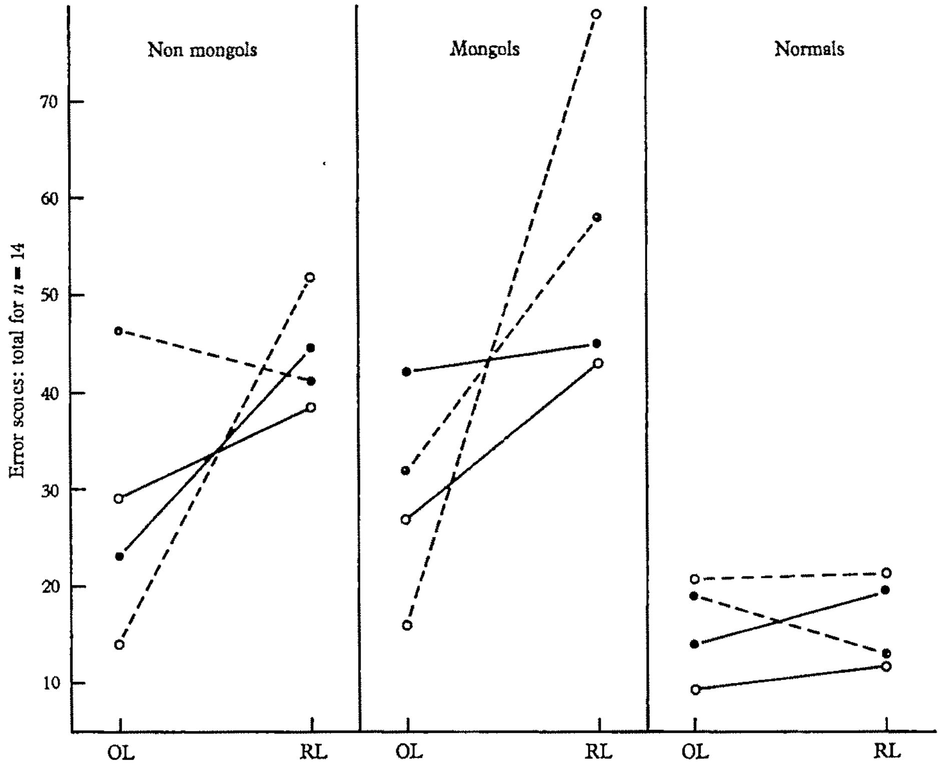

Fig. 3. Criterion effects in both tasks. ●, A; ○, task B; - - -, criterion of six successive correct trials in original learning; ——, 12 correct trials; OL, original learning; RL, reversal learning.

First choices

The first matching choice in original learning of both tasks was recorded for each subject as a shape (S) or colour (C) response. As there were no trends over time from task A to B, subjects were categorized on the combined task data as consistent (SS or CC) or mixed (SC) responders. These data, given in Table 1, show that the mongol group contained a relatively large proportion of colour, and a small proportion of shape, responders. The mongol–normal difference was significant ($\chi^2 = 8{\cdot}11$; $P < 0{\cdot}025$) while the mongol–non-mongol difference fell just short of the 0·05 level.

Preference, as judged by first choice on either task, failed to correlate with CA in any group. In the non-mongol group, shape preference and progressive matrices scores yielded a coefficient which was significant in task A ($r_{p\,\mathrm{bis}} = 0{\cdot}36$; $P < 0{\cdot}05$), and fell just short of significance in task B.

Errors to criterion scores

Groups × conditions totals for errors to criterion, in original and reversal learning of both tasks, are shown in Table 2. Four separate three-way analyses of variance were carried out on these scores, each with three main terms: groups, conditions and criterion.

Table 1. *First choices in tasks A and B: number of subjects making two shape (SS), two colour (CC) or mixed (SC) responses*

Subject group ($n = 28$ for each)	SS	CC	SC
Non-mongols	10	11	7
Mongols	3	14	11
Normals	10	5	13

Table 2. *Error scores: groups by conditions totals*

($n = 28$ for each subject group; first trial discounted in reversal learning.)

	Original learning			Reversal learning			Reversal scores		
	S	C	Group totals	S	C	Group totals	S − C	C − S	Group totals
Task A									
Non-mongols	25	44	69	29	56	85	31	−15	16
Mongols	59	15	74	67	34	101	−25	52	27
Normals	15	18	33	22	11	33	−4	4	0
Conditions totals	99	77	.	118	101	.	2	41	.
Task B									
Non-mongols	14	29	43	42	47	89	33	13	46
Mongols	36	7	43	77	49	126	13	70	83
Normals	7	22	29	24	9	33	2	2	4
Conditions totals	57	58	.	143	105	.	48	85	.

Original learning. In both tasks this analysis yielded a significant groups × conditions interaction ($P < 0{\cdot}025$). Much the same pattern of scores obtained in both tasks; while non-mongols and normals made more errors in colour relevant than shape relevant learning, mongols showed the reverse pattern. In both tasks, t tests showed that mongols made significantly more errors with shape than with colour relevant ($t = 2{\cdot}77$, $P < 0{\cdot}01$; $t = 2{\cdot}29$, $P < 0{\cdot}05$, respectively). Also, in both tasks, mongols made significantly more errors than normals with shape relevant ($t = 2{\cdot}77$, $P < 0{\cdot}01$; $t = 2{\cdot}29$, $P < 0{\cdot}05$, respectively). No other terms in these analyses approached significance.

Reversal learning. Task A analysis yielded a significant groups × conditions interaction ($P < 0{\cdot}025$) due to a similar pattern of group differences to that obtaining in the original learning scores. In the shape relevant condition, mongols made more errors than both non-mongols and normals ($t = 2{\cdot}62$, $P < 0{\cdot}025$; $t = 3{\cdot}11$, $P < 0{\cdot}01$, respectively); with colour relevant, non-mongols made more errors than normals ($t = 3{\cdot}11$; $P = 0{\cdot}01$). Task B analysis yielded a significant groups term ($P < 0{\cdot}001$) mongols making most errors and normals least; t tests showed that both mongol–

normal and non-mongol–normal differences were significant ($t = 3{\cdot}75$, $P < 0{\cdot}001$; $t = 2{\cdot}26$, $P < 0{\cdot}01$, respectively). This order was the same as that in original learning and these significant group differences were probably the result of differences in original learning rate combined with those due to reversal. In this analysis, a significant criterion term ($P < 0{\cdot}05$) was caused by the fact that in the more stringent criterion subgroups, fewer errors were made on reversal; this effect was confined to the mongol and non-mongol groups (see Fig. 3).

Correlation with preference. A preference for the relevant dimension of a task, as judged by first choices, was associated with fewer errors. Correlations were significant for original learning of both tasks ($r_{p\ \mathrm{bis}} = -0{\cdot}76$, $P < 0{\cdot}01$; $-0{\cdot}56$, $P < 0{\cdot}01$ respectively).

Reversal scores

Reversal scores were obtained by subtracting errors in original from those in reversal learning. This score, then, varied directly with the amount of negative transfer which could be due to either or both of two components: (1) a difference in dominance between the dimensions relevant before and after switch, and (2) reversal itself, irrespective of dimension. Two separate, three-way analyses of variance were carried out on the reversal scores for each task; main terms were the same as in the previous analyses.

Both analyses yielded a significant groups × conditions interaction. In task A this was highly significant ($P < 0{\cdot}001$); mongols showed significantly more negative transfer in colour-to-shape than in shape-to-colour reversal ($t = 3{\cdot}48$; $P < 0{\cdot}001$); mongols also showed less negative transfer than non-mongols in shape-to-colour reversal ($t = 2{\cdot}35$; $P < 0{\cdot}025$) and more negative transfer than both non-mongols and normals in colour-to-shape reversal ($t = 2{\cdot}94$, $P < 0{\cdot}01$; $t = 2{\cdot}17$, $P < 0{\cdot}05$, respectively). In the task B analysis this interaction was significant at the 0·05 level and t tests revealed a similar pattern of inter-group differences to that of the task A analysis. Again, mongols showed more negative transfer in colour-to-shape than in shape-to-colour reversal ($t = 2{\cdot}58$; $P < 0{\cdot}025$) and also more negative transfer than both non-mongols and normals in colour-to-shape reversal ($t = 2{\cdot}58$, $P < 0{\cdot}025$; $t = 3{\cdot}08$, $P < 0{\cdot}01$, respectively). The criterion term was also significant in this analysis ($P < 0{\cdot}01$), the more stringent criterion being associated with less negative transfer; as in the task B reversal learning analysis, this effect was confined to the non-mongol and mongol groups (see Fig. 3).

Reversal score data for the tasks were combined and subjected to a mixed design analysis of variance with additional between-subjects and task terms. The groups × conditions interaction was again significant, and was interpreted in this and the separate task A and B reversal score analyses as reflecting group differences in colour–shape dominance. The criterion × tasks interaction was also significant ($P < 0{\cdot}05$) in this analysis, due to the fact that the criterion effect was greater in the more difficult task B.

Verbalization

Data are given in Table 3. For all groups, colour (C) was an easier concept to formulate verbally than shape (S) and colours were more frequently named than shapes. The two tasks differed in respect of both types of verbal data; naming of both colours and shapes was easier in task A than task B; conversely, more concepts were formulated in task B than task A. This was most marked in the case of shape verbalization; in task B, where the naming of shapes was particularly difficult, the number of successful concept formulations was treble that of task A.

Apart from task B naming, which was very difficult for all subjects, these data indicate that mongols, but not non-mongols, showed a verbal deficit, compared to normals. In both types of verbal task the non-mongols equalled or slightly exceeded the performance of the normals and were significantly superior to the mongols.

Table 3. *Number of subjects able to formulate concepts and name all stimuli*

Subject group (*n* = 28 for each)	Task A				Task B			
	Concept		Naming		Concept		Naming	
	S	C	S	C	S	C	S	C
Non-mongols	6	18	13	28	15	21	0	16
Mongols	0	5	5	21	6	8	0	14
Normals	7	21	12	22	15	21	0	13
Group difference significant below 0·05 level	NM–M M–N	NM–M M–N	NM–M	NM–M NM–N	NM–M M–N	NM–M M–N		

DISCUSSION

The main aim of this experiment was to investigate the problem of dimensional dominance in three groups of subjects and the data indicate that the two measures used for this purpose, namely first choices and learning rate, were closely related. The normal group was neither strongly colour- nor shape-dominant and contained a relatively high proportion of mixed responders, as judged by first choices in the two tasks. This was in agreement with the body of evidence to date, which suggests that normal children of this age are in a stage of transition from colour- to shape-dominance. On the basis of learning rate, non-mongols were slightly shape-dominant compared to normals, and although the chronological age range was considerable in both subnormal groups (12–20 yr.), this variable appeared to be unrelated to dominance. In their colour–shape response, the non-mongols showed themselves to be nearer their MA than CA level.

The most striking feature of these results was the strong colour-dominance of the mongol group and the mongol–non-mongol difference, which as a transfer effect (i.e. between original and reversal learning), proved to be considerable. In the absence of any *a priori* reasons for predicting it, this was an unexpected result, and the fact that the subnormal groups were matched, both on CA and a criterion involving the visual perception of shape, makes it difficult to account for their difference in colour–shape response. There was no evidence, moreover, that verbal factors could have contributed to this result; although colour was easier to verbalize than shape, this was the case for all groups alike. The known facts on mongol–non-mongol differences are so few that any attempt to draw on them for an explanation can only be speculative. O'Connor & Hermelin (1963), in a series of studies on mongol–non-mongol differences, found that these two groups did not differ in tasks of visual perception; they did, however, find that mongols showed a relatively greater number of eye movements under certain conditions of illumination, and were also impaired in stereognostic recognition tasks and certain visuo-motor tasks, such as reproduction of designs. These authors suggest that such deficits could be attributed to a cerebellar impairment, hypotonia and lack of kinaesthetic feedback in the mongol. If, as their findings indicate, mongols are handicapped spatially through the modalities of touch and proprioception, it seems not unlikely that a lag in the progression from colour- to shape-dominance could result. Such a supposition does rest on an assumption of intermodal transfer but there is some evidence that modalities other than the visual can affect the colour–shape response. Doehring (1960) and Gaines (1963) have found that deaf children of 8–12 yr. are colour-dominant at an age when normal children have been found to be shape-dominant. It is also relevant to mention here the theories of Hebb (1949) on visual perception, according to which the early stages of shape recognition involve visual tracing of outline and the use of voluntary muscle. If the mongol is indeed subject to a general kinaesthetic handicap, the question arises as to whether other spatial dimensions in the visual modality, such as size or height, would be similarly affected.

The subjective impression gained was that the normal children used more spontaneous verbalization of both concepts and stimulus names, whereas the subnormal subjects rarely verbalized spontaneously, particularly at a conceptual level; when they did so, it was as often as not either inappropriate (referring to an irrelevant dimension) or incorrect. The verbal data on the whole suggest that in the mongol group verbal mediators were rarely available, while in the non-mongol group they were available but not used for task solution. In both cases, however, these subjects were able to form concepts at an operational level in a fairly efficient manner. Such comments would be in accordance with those of other workers on the discrimination learning behaviour of subnormal subjects generally.

A particularly interesting finding was the superiority of the second, more difficult task in the amount of successful concept formulation elicited. Failure to formulate a concept was not corrected in the first task, and the concept word was given to the subject once only, when he was asked to name stimuli. It seems unlikely, therefore, that this was a verbal transfer effect from first to second task. If the ability to formulate a concept verbally is a function of task difficulty, it follows that the level of verbal ability, both required and attained, would be greater in the second than the first task. A further possibility is that specific labelling represents a lower level of verbalization than concept formulation and, as a lower level response, will tend to be produced more readily than the higher level response, even when the latter is available. Thus, in the second task, more subjects were forced into the overt formulation of concepts by the fact that specific labels were not available to them. That the majority of these subjects, with the exception of some mongols, knew the meaning of the words 'shape' and 'colour' was evident from their ability to name at least one of the stimuli, however incorrectly, when given these words as a cue. To incorporate the word in an overt response, however, would appear to require a higher level of ability. This raises the question of what factors affect the availability of verbal mediators and, with reference to the training of subnormal subjects, those inherent in the task situation are of particular interest. It has been found, for instance, by

Klugh & Janssen (1966), that successive presentation of stimuli is superior to simultaneous presentation in producing successful verbalization in subnormals.

The finding that, in both subnormal groups, reversal was accompanied by overall negative transfer, supports the view that pre-criterion learning was predominantly controlled by the acquisition of a relevant dimensional or 'observing' response. It was also found, for both subnormal groups, that subjects who had learned to a more stringent criterion showed less negative transfer. This effect (see Fig. 3) was not statistically a strong one, and examination of the data suggests that a sampling error could have been partly responsible; original learning rate was considerably (though not significantly) more rapid in the less stringent criterion subgroups, and if this rapid learning was due to a dominance of the relevant dimension it would be followed by slow learning in the reversal task, where such dominance would be a hindrance. A more interesting possibility is that this was a genuine overlearning effect and that learning during the additional criterion trials involved some process other than the acquisition of a relevant dimensional response. The acquisition of an irrelevant dimensional response during this period would account for facilitated reversal performance; the latter effect, however, would only be expected where the irrelevant dimension of original learning was a non-dominant one and this was not, in fact, the case. Alternatively, by a process of response integration, a higher level strategy was formed which incorporated both dimensions and differential responding between them. Such a strategy would resemble those referred to by Bryant (1965) and also the 'analogic structure' of Mandler (1962). According to the latter author, such structures are formed only when errors are eliminated and the response sequence becomes relatively stable, as in overlearning trials. The facilitating effect of such a structure on transfer is said to be due to the fact that it allows covert trial and error behaviour in the transfer task.

In contrast to both subnormal groups, normals showed zero rather than negative transfer on reversal, and no overlearning effect. The fact that original learning rate in this group was more rapid, and therefore nearer an optimal level, would preclude the possibility of facilitation due to overlearning. It would not, however, explain the zero transfer on reversal, which suggests that, at the start of reversal learning, the normal subjects were in the possession of a strategy appropriate to both original and reversal learning alike. That such a strategy could have been the result of pre-experimental experience seems unlikely in experimentally naïve subjects. More probably it was acquired at an earlier stage of original learning in the case of the normal subjects. This would constitute a qualitative difference between groups in the nature of the learning process; one reason for this difference could be that the more rapid emergence of higher-level strategies was favoured in the normal group by the relatively efficient operation of verbal mediators.

One further possibility is that difficulty level may have been the decisive factor in determining both original learning rate and the qualitative nature of the learning process. The general transfer effects found in this experiment and, in particular, the fact that these effects were appreciably greater in the second, more difficult task, support this view. The relation between task complexity, or difficulty, and transfer has been the subject of a recent series of studies by A. D. B. Clarke & Cooper (1966), A. M. Clarke *et al.* (1966) and A. M. Clarke & Cooper (1966); these present evidence that, in both normal and subnormal populations, transfer across conceptual learning tasks is a direct function of training task complexity and independent of starting levels.

The implication of the present findings, in the first instance methodological, is that it is not enough to accept matching procedures which produce statistically non-significant differences in initial performance. A recent trend has been to follow the practice in animal work and use some criterion measure based on discrimination learning performance itself. These results would indicate that, to be effective, such a measure must be dimension-specific. In relation to further research, the significance of this experiment has been to show that dimensional dominance (possibly through its effect on difficulty level) could be a major source of experimental error, and that valid comparisons between populations can only be based on procedures which aim at its control.

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THE NON-RANDOMNESS OF ATTEMPTS AT RANDOM RESPONSES: RELATIONSHIPS WITH PERSONALITY VARIABLES AND PSYCHIATRIC DISORDER

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Studies have shown that subjects are not random in their responses, even when attempting to achieve randomness. In this study, normal and schizophrenic subjects were asked to give numbers from one to nine at random and paced. They were given an interpolated task, and then retested. Some statistical tests of degree of randomness were devised and it was shown that no subject was random, but that stereotypy decreased within and over test–retest. The results are discussed in terms of learning and extinction of stereotypy. Differences between introverts and extraverts were almost significant in the opposite direction from that predicted. Schizophrenics were significantly more stereotyped than normals. Finally, it was shown that for a normal subject, his test could be identified from other subjects' tests on the basis of retest protocol, the test protocol providing a kind of psychological 'fingerprint'.

A number of writers have discussed the inability of human subjects to generate random sequences of choices from a finite set of responses. Tune (1964*b*) gives a review of the literature on evidence of response preferences, and puts forward the hypothesis that the non-randomness is due to limitations of short-term memory (Tune, 1964*a*). Weiss (1964) also gives a review and criticizes Tune's hypothesis of a limitation of short-term memory on the grounds that the problems of encoding and storing the necessary frequency information, even in short-term memory, are overwhelming (Weiss, 1965). He states that the use of memory devices may lead to even less success than is usual for most subjects. Weiss further points out that, according to the memory hypothesis, it would be most difficult for subjects' responses to become less stereotyped with time, as more and more information has to be stored. He suggests that in any alphabet of symbols some of the combinations of symbols will be dominant over others, and to randomize successfully a sequence of symbols the subject must inhibit attention to each response after it occurs.

The present experiment set out firstly to test whether subjects become more or less random over time, and in this way to test the short-term memory hypothesis proposed by Tune. A second purpose of the experiment was to investigate possible differences in degree of randomness due to personality differences. Weiss (1964) remarked on large individual differences and suggested the need for investigation of the effects of organismic variables. Eysenck (1960) postulated that extraverts build up inhibitory potentials (both reactive inhibition, I_R, and conditioned inhibition, $_SI_R$) faster than do introverts. A paced task such as giving random numbers at a certain speed would result in a build-up of inhibition, and, if there are patterns within the protocols, these should extinguish faster in the case of extraverts than in introverts. Thus extraverts should show a greater degree of randomness over time. The finding that chronic schizophrenic subjects differ from normal subjects, i.e. they are slower than normal in the rate of decay of reactive inhibition (Venables, 1959; Broadhurst & Broadhurst, 1964), enables corresponding differential predictions to be made for these groups in the random-generation response. Though not investigated here, the suggestion has been put forward that differentiation of schizophrenics of different types (process and reactive) by their random-generation behaviour is possible (Zlotowski & Bakan, 1963). A further purpose of the experiment was to test a hypothesis put forward by Sekey (1964) that, since subjects are not random in generating numbers, the possibility might arise of identifying a particular individual by his matrix, i.e. a sort of psychological fingerprint.

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METHOD

The subjects in the first group were 72 male paid volunteers who came to the Maudsley Hospital to act as subjects in psychological experiments. The average age was 30 yr. (range 16–53 yr.). The subjects were instructed:

'Take the series of numbers 1 to 9 (1, 2, 3, 4, 5, 6, 7, 8 and 9), and say numbers from this series, any numbers, in any order, giving the numbers in time to the metronome. For example (metronome started) 9, 5, 2, 6, 3 and so on. Make your choice of numbers as varied as possible, and just keep on saying any numbers you like till you are told to stop.'

The metronome (set at 1 beat per sec.) was started and as the subject gave the first number the stopwatch was started and allowed to run until 300 numbers had been given—5 min. if there were no rest pauses. The experimenter sat at a side table recording each number (excluding 0 and those above 9) as it was given, and subjects could not see the records of their responses. An interpolated task lasting about 45 min. was given, and then the subjects were again asked to produce 300 random numbers.

A group of long-stay chronic schizophrenics in a Birmingham hospital was similarly tested. Of 35 patients tested, 12 followed instructions and gave protocols complete enough for analysis of the results. The average age of the 12 schizophrenics was 41·7 yr.; the mean length of stay in hospital was 9·75 yr.* All subjects completed the Eysenck Personality Inventory (Eysenck & Eysenck, 1964) prior to testing.

Scoring of protocols for degree of randomness

Three different methods were used to assess the degree of randomness:

(*a*) A frequency tally of the number of times each figure was given. This was compared with the expected frequency of occurrence by chance, and a χ^2 calculated.

(*b*) Each number had the subsequent number subtracted from it, i.e. a first-order sequence of difference score, giving 17 possibilities from -8 through 0 to $+8$. The frequency with which each possibility occurred was tallied and compared with the theoretical expected frequency, the score being expressed as a χ^2. For example, if the first five numbers given were 7, 3, 4, 9, 6, the 3 is subtracted from the 7 and a tally placed in the $+4$ cell. The next tally would be placed in the -1 cell, the next -5, etc. If the sequence were truly random, the observed cell frequencies would be close to the expected frequency of

$$(1/9 \times 1/9 \times 299),\ (1/9 \times 2/9 \times 299) \ldots (1/9 \times 9/9 \times 299) \ldots (1/9 \times 1/9 \times 299).$$

(*c*) A 9 × 9 matrix was constructed and a tally placed in the appropriate cell corresponding to each couplet in the series. This can be called a matrix of first-order dependencies. For example, from the numbers given above, a tally is placed in the cell 7/3, the next tally in cell 3/4, then 4/9, etc. If the sequence were random, the cell frequencies would all be close to the expected frequency of 299/81 or 3·7. A χ^2 can then be calculated, but this will tend to be inflated as the expected cell frequencies are less than 5 (Siegel, 1956).

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* We are indebted to Dr N. W. Imlah and the staff of All Saints Hospital, Winson Green, Birmingham for their co-operation in this phase of the experiment.

All test and retest protocols were scored by all three methods, then each test and retest was split into sequential halves for comparison of the first 150 numbers given with the second 150 numbers. All calculations were programmed for computer processing. A bogus protocol taken from a table of random numbers was also scored by the above methods.

RESULTS

The results of the different scoring methods are summarized in Table 1, which gives the results of *test*, rather than retest, scores. It can be seen that the protocol taken from the table of random numbers in all scoring methods was within the range of scores expected by chance, while the test protocols of all subjects on the last two methods of scoring showed large deviations from randomness. The schizophrenic group showed greater stereotypy or less randomness than the normal group, and this stereotypy reached significance in all measures for the abnormal group (see Table 1).

Table 1. *Test scores (χ^2) resulting from the different methods of assessing randomness*

	Frequency tally*	Difference tally**	Matrix tally***
χ^2 score of protocol from table of random numbers	6·36	19·59	77·83
Average χ^2 score for normal group	10·31	131·78	240·72
Range of normal group	0·54–34·44	32·01–364·26	99·50–515·06
Subjects beyond 1 % level	11 %	100 %	100 %
Average χ^2 score for schizophrenic group	30·94	541·62	772·37
Range of schizophrenic group	0·18–201·36	29·67–2,101·36	160·72–2,118·30

* χ^2 significant at 0·01 level = 20·09.
** χ^2 significant at 0·01 level = 32·00.
*** χ^2 significant at 0·01 level = 94·40.

For further statistical analysis of the results it was decided to work only with the difference scores, as these showed a highly significant deviation from randomness, measured almost the same thing as the matrix scores (correlation between difference and matrix scores was 0·915), were more stable because of the greater expected cell frequencies, and showed less heterogeneity of variance than the matrix scores. The difference scores showed heterogeneity of variance between different personality groups (overall mean $\chi^2 = 131{\cdot}78$; $s^2 = 5{,}554{\cdot}73$) so a square-root transformation of the scores was taken for further analysis. The scores decrease from test to retest, and from first half to second half, but in an analysis of variance the sequential halves test–retest negative linear trend was not significant ($F = 2{\cdot}37$; 1, 284 d.f.; $P > 0{\cdot}05$). Thus stereotypy decreases and randomness increases over time but not significantly so for all subjects. Taking the scores for the 72 normal subjects, the analysis of sequential halves test–retest negative linear trend was highly significant ($F = 9{\cdot}26$; 1, 213 d.f.; $P < 0{\cdot}005$).

To examine the extent to which the personality factors of extraversion and neuroticism account for some of the inter-subject variance, 52 of the 72 normal subjects were selected to fall conveniently into the four quadrants of high and low extraversion with high and low neuroticism. The average extraversion and neuroticism scores for each quadrant were E = 17·6 and N = 5·4; E = 17·2 and N = 16·9; E = 7·4 and N = 4·9; and E = 6·9 and N = 15·0. The linear trend interaction of extraversion by neuroticism by test–retest sequential halves reached significance (F = 4·33; 1, 144 d.f.; P < 0·05), and the linear trend of extraversion by test–retest sequential halves also reached significance (F = 4·27; 1, 144 d.f.; P < 0·05). The extraverts tended to have generally higher scores than the introverts, and on retest the stereotypy of the extraverts increased rather than decreased, and introverts decreased as shown in Fig. 1. When considering personality variables it is of interest also that schizophrenic stereotypy is not only significant, as noted above, but also significantly greater than normal for the difference scores (Table 1) (t = 2·30; 82 d.f.; P < 0·05). The schizophrenic test–retest sequential half scores are shown in Fig. 1 for comparison with the introvert–extravert scores.

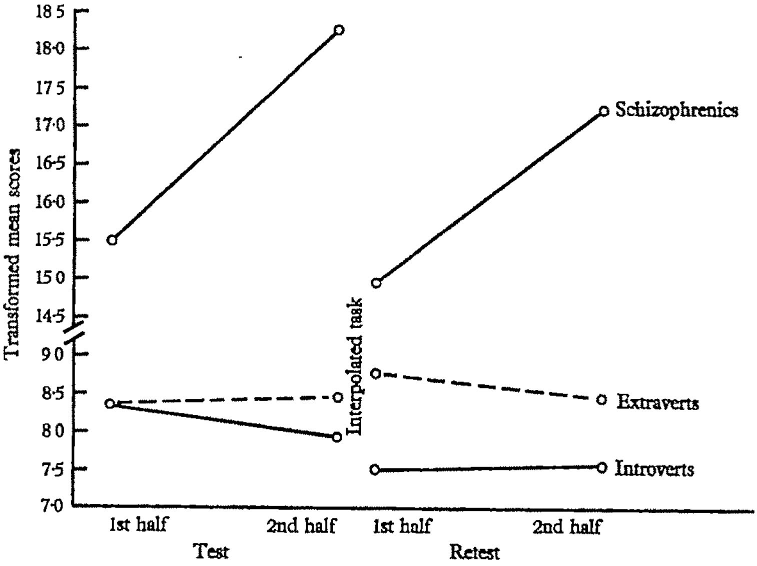

Fig. 1. Average difference scores on sequential halves of test–retest for introverts, extraverts and schizophrenics.

To test whether a single subject's test protocol could be identified from other protocols on the basis of his retest, one subject was chosen at random and his retest compared with his test and also with the test protocols of four other subjects, all chosen at random. The retest–test matrices were compared and scored for similarity on the basis of squaring the difference between cell frequencies in the two matrices. The scores were as follows: retest–test: 478; retest compared with four other subjects' tests: 620, 776, 830 and 846. On this basis, the subject's retest resembles more closely his test than it resembles the tests of the other subjects. This part of the analysis was not done by computer and is laborious, and so was only repeated for two other subjects chosen at random and compared in each case with four other subjects, all chosen at random. In each case the subject's retest–test score was the lowest score. Thus an individual's test protocol can be identified from five protocols, four of which have been given by other subjects chosen at random.

This identification of a single subject's protocol on the basis of his retest matrix was not maintained for the schizophrenic group. A schizophrenic subject chosen at random had a test–retest score of 2636, and compared with four other schizophrenic subjects the scores were 1540, 2364, 3290 and 4506.

DISCUSSION

In accord with other studies on randomness of response in human subjects, the results of this experiment show that even when subjects try to be random, there is a high degree of stereotypy. The results also show that stereotypy decreases over time within a short testing session, and also from test to retest. As short-term memory would become less efficient over time with more information to process, Tune's (1964*a*) hypothesis (of randomness limited by short-term memory) must be modified. Weiss (1964) found, in comparing the first half with the second half of his protocols of 'random' responses, that stereotypy increased in the second half—a finding at first glance contrary to those of this experiment. But Weiss used binary numbers, and he found a shift towards greater simplicity of patterning. Thus his results and those given above where stereotypy decreases over time are not necessarily contrary to each other.

Weiss (1965) hypothesizes that in order to maintain effectively a random sequence one must suppress or otherwise inhibit paying attention to each response after it occurs, and he suggests (Weiss, 1964) that stereotypy be viewed in the framework of set and attention. An alternative framework, not inconsistent with that put forward by Weiss, is to consider stereotypy in terms of learned habits and their extinction. All subjects appear to have preferred combinations of numbers which occur more often than would be expected by chance. Further, each subject appears to have preferred combinations which are different from those of other subjects. It would seem reasonable to assume that these combinations are learned during an individual's life-history. Such things as house numbers, telephone numbers, car registration numbers, personal identity numbers, etc., would result in idiographic digit combinations being learned. Now if a subject is asked to produce numbers at random at a certain rate, the combinations previously learned will be used more than would be expected by chance. But, while in a life situation there are definite social reinforcements for using certain combinations, in a random test setting there will be little or no reinforcement for these combinations and they will therefore tend to extinguish—resulting in less stereotypy and more randomness. It can also be argued that repetition of familiar combinations will result in a build-up of reactive inhibition (I_R) for these combinations which would lead to a reduction in their occurrence with time. This change in degree of randomness does in fact occur in the results.

If we are dealing with extinction, and if we consider Eysenck's (1960) hypothesis that extraverts extinguish faster than introverts, we would expect the curve for extraverts to drop more steeply over time. In fact, the extravert–introvert differences were not significant but the linear trend was significant in the opposite direction (Fig. 1). Discussion of the results in these terms must be tentative in view of the lack of significance in our data and as the whole concept of extinction in learning is questionable (Kimble, 1961; Slater & Yatkin, 1965), and, particularly in relation to extraversion–introversion, remains open to experiment. However the extreme stereotypy of schizophrenics on these measures is in line with predictions based on their exaggerated inhibitory potential as well as confirming clinical impressions.

The hypothesis put forward by Sekey (1964) that it would be possible to identify a particular individual by his random numbers matrix seems to hold for normal subjects but not for schizophrenics. Sekey further suggests that possibly a pathological configuration of a matrix can reveal a mental disease. The comparison of normal with schizophrenic scores in this study showed a highly significant difference, and Thomas (1962) has used protocols of random numbers to differentiate brain-damaged patients from normal subjects. Yacorzynski (1941) also found that when subjects were required to predict a random binary choice, normals preferred non-symmetrical patterns while schizophrenics and manic depressives were more symmetrical and stereotyped. Therefore it seems reasonable to state that it would be possible to pick out pathological protocols from normal protocols, but it is not known with what degree of accuracy.

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DEVELOPMENTAL CHANGES IN SOME COMPONENTS OF A MOTOR SKILL

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Sixty children, aged 6, 8 and 10 yr., performed a target task 12 times. The results were analysed in terms of speed, accuracy and scatter. In each of the three age-groups the girls performed significantly faster than the boys. Older children were faster than younger ones. No reliable age or sex differences were found in the accuracy component. Speed improved with practice, accuracy did not. The effect of practice was different in the 6 yr. group compared with the 8 and 10 yr. groups. The scatter of shots around the target was found to differ progressively with age and was interpreted as evidence of a change in strategy reflecting the development of a fine motor skill.

As Munn (1965) points out, relatively little research has been carried out in the last decade on the development of motor skill in children. Much of the more recent work has been undertaken by physical educators and has been directed towards discovering the age at which certain motor activities should be taught. A great deal of this work is descriptive and although large amounts of normative data have been published, for example the behaviour profiles of Shirley (1931) and Gesell & Ilg (1946), little is known of the changes in the various components of skilled motor performance as development proceeds.

Jones & Seashore (1944) described fine motor skills as being revealed 'through deftness and accuracy in limited movements'. A number of studies have examined developmental changes in speed of response, though few have considered both accuracy and speed. Bryan (1892) used tapping as a measure of 'speed of muscular movement' and found that the gain in speed as a function of age was approximately linear to the age of 16. Goodenough & Brian (1929), Goodenough & Tinker (1930) and Goodenough (1935*a*) have made careful measures of speed of tapping on a keyboard and with a stylus. They report a steady improvement with age and differences between the two hands and various fingers.

Despite the paucity of recent research on the development of motor skill in children, an impressive amount of work has been undertaken with healthy adults and ageing subjects (Bilodeau, 1966; Welford & Birren, 1965). Experimental analyses of the kind reported by these authors where motor responses have been broken down into the constituent components have led to the development of several models of sensory-motor performance. These have been evaluated by Welford (1960).

The experiment reported in this paper was undertaken in order to analyse the changes in speed and accuracy of a simple motor response as development proceeds.

METHOD

Subjects. Sixty children in all were used in this study. Ten boys and ten girls drawn from each of the following three age-groups: (1) mean age 5 yr. 11 mth., range 5 yr. 6 mth. to 6 yr. 6 mth.; (2) mean age 7 yr. 10 mth., range 7 yr. 6 mth. to 8 yr. 3 mth.; (3) mean age 9 yr. 8 mth., range 9 yr. 4 mth. to 10 yr. 3 mth.

For convenience these will be described as the 6, 8 and 10 yr. old groups respectively. The subjects were drawn from two schools whose catchment area drew on populations with similar socio-economic backgrounds.

Materials. Two circles each with a radius of 1 in. with centres 5 in. apart, were drawn on a stencil; the centres were not marked. Copies were then produced on sheets of paper 8 × 5 in. While a trial was in progress the paper was held on a board 11 × 9 in. by two rubber bands, which kept it firmly in position and prevented slipping. This arrangement allowed a quick change of paper between trials and ensured that the same position was maintained throughout the experiment.

The pencils used by the children were cut down to 5 in. in order to prevent them being unwieldy, particularly for the younger children. An adjustable chair was used to control the height of the children relative to the desk. The trials were timed with a stopwatch.

Procedure. The subjects were tested individually. Each was given a pencil, and a sheet of paper with the two circles on it was clipped to the board. The children were introduced to the task in the following way. The child was shown the target sheet and asked, 'Do you see these two circles?' Following an affirmative reply the instructions continued. 'Good! Now I want you to dot your pencil backwards and forwards from one circle to another like this.' The experimenter then demonstrated the task. 'Try to put your dots as near to the middle as possible and do it as quickly as you can. Alright, you try. Hold your pencil over a circle and start when I say "Go". Keep going as fast as you can until I say "Stop".'

Each child was given one sheet for practice which continued until it was clear that he understood what was required in terms of the two components of speed and accuracy. Following this, a clean target sheet was attached to the board and the instructions continued. 'That's fine. Just what I wanted. Now let's see how many dots you can make this time. Ready? Go.'

If the dots made by a child became rather widely aimed and he began to place more than one or two per trial outside the circles, he was reminded that accuracy mattered as well as speed. The stopwatch was started as the first dot was made and a period of 5 sec. was timed. Any dots which were made after the child was instructed to stop (this occurred not infrequently with the younger children) were marked to indicate that they should not be included in the analysis.

Each child performed the task 12 times and each sheet was marked with a code identifying the child and the trial number.

The children were given knowledge of results after each trial and encouraged with remarks such as, 'Good, you made 16 dots that time: let's see what you get next time.'

It was not expected that fatigue would develop to any appreciable extent since the task was so selected that it involved only short bursts of effort rather than a long period of sustained effort. However, every child was given a short rest period of 3 min. after the sixth trial. During this rest period the experimenter chatted to the children and obtained some feedback about their opinion of the task.

Statistical analysis. In all, 720 target sheets were available for analysis, one for each of the 12 trials carried out by each of the 10 subjects of each sex in the three age-groups. Two scores were extracted from each target sheet, representing the achievement of the subject in that trial for each of the basic components of speed and accuracy of performance. In addition, the target sheets used in the second, seventh and twelfth trials of each subject were examined further to obtain information about the scatter of the dots recorded.

The basic design of the experiments involves therefore the factors of age, sex and trials as well as subjects. A repeated measures analysis of variance, based on a paradigm in Winer (1962), was employed. A slight modification was introduced into the analysis to assess the effect due to practice, i.e. the change, if any, in performance over the course of the experiment. This must be distinguished from the trials effect, which takes no account of the order in which the trials were carried out. The variation due to practice was therefore assessed as being that portion of the variation due to differences between trials attributable to a linear regression on the number of the trial. The remainder is referred to in the tables as the effect due to 'Trials: non-linear'.

A fixed effects model was taken throughout, except for the effect due to subjects within age and sex groups, which was taken as random.

RESULTS

Speed of response

The score for speed was obtained from the total number of dots recorded in a particular trial, this being recorded for each subject on every trial. The mean number of dots recorded for boys and for girls in each age-group is shown in Table 1.

Table 1. *Mean number of dots per subject per trial for each age and sex group*

Age (yr.)	Male	Female	Both sexes
6	11·40	13·41	12·40
8	15·73	17·53	16·63
10	20·21	23·04	21·63
All	15·73	17·99	16·88

Table 2. *Analysis of variance for speed of response*

Source	D.F.	M.S.	*F*	Significance
Between subjects				
Age	2	5113·56	45·31	***
Sex	1	882·23	7·85	**
Age × sex	2	17·92	< 1	n.s.
Subjects (error)	54	112·35		
Within subjects				
Trials: linear	1	734·37	408·21	***
non-linear	10	2·671	1·48	n.s.
Age × trials: linear	2	71·93	39·78	***
non-linear	20	1·115	< 1	n.s.
Sex × trials: linear	1	0·040	< 1	n.s.
non-linear	10	1·104	< 1	n.s.
Age × sex × trials: linear	2	6·435	3·58	*
non-linear	20	0·916	< 1	n.s.
Subjects × trials (error)	594	1·799		
Total	719			

* $P < 0·05$; ** $P < 0·01$; *** $P < 0·001$.

The analysis of variance referred to above was carried out on the scores and a summary of the results is shown in Table 2. It can be seen that the effects due to age, to sex and to practice were found to be significant, as were the effects of interaction between age and linear trials and between age, sex and linear trials.

The significant main effects show clearly that girls are superior to boys in terms of speed, that older children tend to perform faster, and that there is a practice effect. In this case there is a positive correlation between speed and the number of the trial, so that the effect of practice is to increase speed. The significant interaction between age and linear trials shows that the differences in the results on different trials is not the same for all age-groups. The extent to which this is so can be seen clearly in Fig. 1, which also shows the form of the three-factor interaction between age, sex and linear trials. It is apparent from Fig. 1 that the 6-yr.-old children do not alter their performance with practice to the same extent that the older children do.

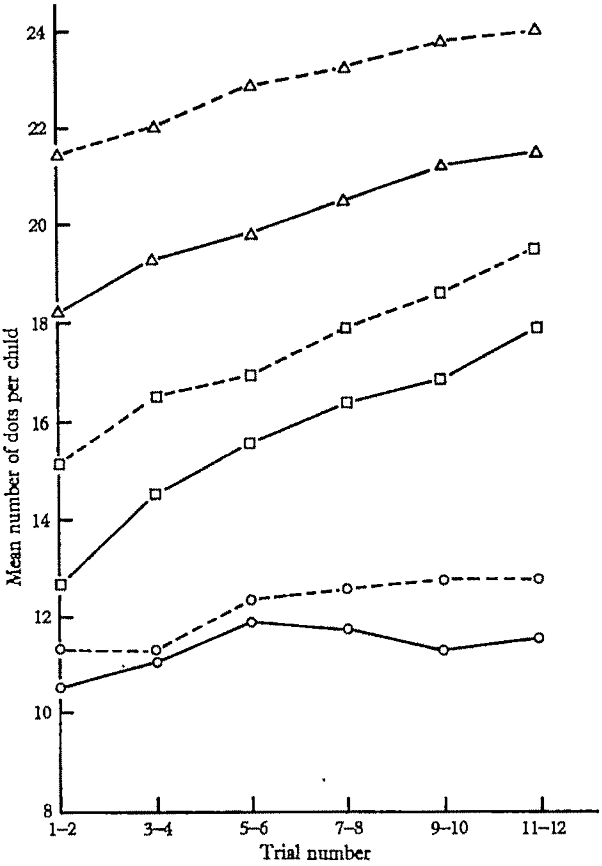

Fig. 1. Mean number of dots made by boys and by girls in the three age-groups over the 12 trials. Trials 1 and 2, 3 and 4, and so on, are combined. – – –, girls; ——, boys; △, 10 yr. group; □, 8 yr. group; ○, 6 yr. group.

Accuracy

The subjects were instructed to aim at the centre of each circle; a measure of accuracy, therefore, is the distance of a dot from the centre of the circle at which it was aimed. Because of the large number of dots, this was simplified by assigning a score to each dot. The circles were divided into four sections by drawing within them concentric circles of radii $\frac{1}{4}$ in., $\frac{1}{2}$ in. and $\frac{3}{4}$ in. Dots inside the middle section were awarded a score of 4, those between $\frac{1}{4}$ in. and $\frac{1}{2}$ in. from the centre a score of 3, and so on. Dots falling outside the circle at which they were aimed received a score of zero. The overall measure of accuracy was then found by dividing the total score for both circles combined by the total number of dots. This measure has a monotonic relationship with the mean distance of dots from the centre, for all reasonable distributions of dots. The measure might break down if a large proportion of the dots fell inside the centre section or outside the circle; however, no subject approached such extremes.

The accuracy score as described was obtained for each trial of every subject and these were combined to give mean accuracy scores for each age and sex group. In these calculations equal weighting was given to each accuracy score (rather than the scores being weighted by the number of dots made); the results are shown in Table 3.

An analysis of variance, using the model described above, was carried out on the basic accuracy scores, and the results are summarized in Table 4.

Table 3. *Mean accuracy score per subject, per trial for each age and sex group*

Age (yr.)	Male	Female	Total
6	2·93	2·94	2·94
8	2·78	2·89	2·83
10	2·93	2·68	2·80
All	2·88	2·84	2·86

Table 4. *Analysis of variance table for accuracy score for each subject in each trial*

Source	D.F.	M.S.	*F*	Significance
Between subjects				
Age	2	1·1411	1·08	n.s.
Sex	1	0·2598	< 1	n.s.
Age × sex	2	2·1013	1·99	n.s.
Subjects (error)	54	1·0537		
Within subjects				
Trials: linear	1	1·8027	20·61	***
non-linear	10	0·0763	< 1	n.s.
Age × trials: linear	2	0·1101	1·26	n.s.
non-linear	20	0·0617	< 1	n.s.
Sex × trials: linear	1	0·0027	< 1	n.s.
non-linear	10	0·0238	< 1	n.s.
Age × sex × trials: linear	2	0·1792	2·05	n.s.
non-linear	20	0·0775	< 1	n.s.
Subjects × trials (error)	594	0·0875		
Total	719			

*** $P < 0{\cdot}001$

The only effect showing significance is that due to linear trials. The effects of age and sex are not established and it can be seen from Table 3 that this is the only reasonable conclusion since the differences are not large. It is also apparent from this table that the older children do not, if anything, achieve a score as high as the younger children.

The effect of practice (linear trials) is also somewhat unexpected. The analysis shows this to exist, but as the correlation between the accuracy score and the number of the trial is negative the effect of practice is shown to be a deterioration in accuracy.

In general, the results for accuracy show that the underlying variability between children and between the different trials for individual children is so large that it dwarfs any possible difference between the sexes or changes as a function of age.

Scatter

From observations of the children as they performed the task it was apparent that some employed a technique superior to that of others. The children who made fewest dots in the trials tended to waste time in two main ways: they lifted the pencil in a high arc as it passed between the two circles, and they tended to look from one circle to another in time with the movement of the pencil. They tended in fact to treat the task as a series of discrete movements rather than as a continuous whole. By way of contrast, children who made more dots in the time allowed tended to move the pencil in a low arc over the paper, thus reducing the distance travelled. They tended also to concentrate on watching one circle, once they had gained a fair idea of the hand and wrist movement required to reach the other target, checking the accuracy of the dots they were making only periodically. In addition they appeared to treat the task as a co-ordinated whole by touching the pencil down on each circle only lightly so that it was easy to change the direction of motion. This, combined with the low arc of flight over the paper, tended to produce a characteristic side-to-side movement.

These observations suggested that a study of the distribution of the dots within targets would be of value, especially as visual examinations of the targets further suggested that the distribution of dots appeared to differ between age-groups. For the youngest children the dots often had a roughly circular distribution about the centre of the circles, whilst for the older children the spread along the direction of motion often appeared to be greater than that along the perpendicular to this direction. Three trials (the second, seventh and twelfth) were selected for further analysis, and the position of every dot made by each subject in each of these trials was recorded relative to the centre of the circle at which it was aimed. Co-ordinate axes were drawn along the line joining the centres of the circles and through the centre of the circles perpendicular to this line, the former being labelled x (horizontal, positive direction to the right) and the latter y (vertical, positive direction away from the subject). The co-ordinates (x, y) of each dot were recorded to the nearest $\frac{1}{20}$ in.

In terms of these co-ordinates and the distribution of dots within the circles, the observations made in the course of the experiment and noted above focus attention on the variances of the co-ordinates of the dots, considered here as being a random sample from a population of possible dots. In particular, the relative value of the variance of the x co-ordinates and that of the y co-ordinates is of interest. If the spread in the horizontal direction is greater than that in the vertical direction, then $\text{var}(x) > \text{var}(y)$.

Accordingly, a test was carried out for each subject and for each of the three selected trials of the null hypothesis that the two population variances (of x and y) were equal, against the one-sided alternative that the variance of x was greater than that of y. The sample variances were calculated over both circles together and an F-test applied. The use of this test assumes that the population of dots in each circle has a bivariate normal distribution.

The results for the twelfth trial are summarized in Table 5, which gives the number of subjects out of the ten in each category for whom the hypothesis that the x and y variances are equal is rejected at the 95 per cent level. These results show an interesting difference between age-groups and suggest not only that a different pattern of movement develops, but also that a different set to the problem may be present in the older children.

An attempt was made to identify those subjects making clear use of the other aspect of the superior techniques described above; that is, attending primarily to one of the circles. A consequence of using this technique might be that the dots in one circle were more accurately placed than those in the other, and accordingly a null hypothesis was set up that, for a particular subject and a particular trial, the accuracy would be the same for both circles.

Table 5. *Number of subjects out of ten in each group who exhibited evidence of using 'side to side' technique in the twelfth trial*

Age (yr.)	Male	Female	Total
6	5	2	7
8	6	9	15
10	9	10	19
Total	20	21	41

A number of tests are available for this hypothesis, the power of each depending on the type of alternative considered. A likely alternative hypothesis, bearing in mind the fact that subjects who were not attending to the two circles equally would be likely to be employing the other (side-to-side) aspect of this superior technique, would be that the ratio of the variance of x to the variance of y would be greater for the circle not being watched continuously than for the other. In fact, a test based on this hypothesis did show greater power than a number of others tried, but no conclusive results emerged. This was partly because the number of dots made in each circle in each trial was seldom large enough to provide adequate evidence to reject the null hypothesis. However, sufficient evidence did emerge to convince the authors that this possibility was worth further consideration in subsequent experiments.

Practice

For the purpose of the analyses practice was included as a source of variation. A number of interesting features emerged when successive trials were examined. In the analysis for speed of response there was a straightforward increase, attributable to the effects of practice, through trials 1–12. This was examined for the age/sex groups separately, all of which show signs of improvement in the course of the experiment. Fig. 1 indicates that the improvement shown by the 8 and 10-yr.-old groups is greater than that shown by the youngest group, a finding borne out by the results in Table 2. This also shows that the interaction between age and practice is not the same for both sexes though a complete interpretation of this result is difficult.

There was no corresponding increase in accuracy with practice. Most of the age/sex groups showed a small decline in accuracy in the course of the experiment. This is perhaps not a surprising finding since speed and accuracy are generally compensatory.

DISCUSSION

It is unlikely that differences in motivation between age-groups led to the differences in speed of response. All the children appeared to put a good deal of effort into performing the task as fast as they could within the limits imposed by the constraint of accuracy. The increase in speed shown by all groups, and by virtually all children individually, over the 12 trials supports this view. Davol, Hastings & Klein (1965) point to the difficulty of sustaining an adequate level of motivation in studies of motor skills in young children. However, in this investigation the children appeared sufficiently motivated by competing against themselves, at least for the short experimental session involved.

As Stone & Church (1957) point out, 'Boys and girls display considerable disparity in their tastes and interests. It is likely that children might often enjoy activities assigned to the opposite sex but are restrained by a sense that to do so, or even visibly to want to do so, would be a betrayal of their sex role.' From conversations with the children it was clear that this was not considered to be a task exclusively for boys or for girls. In addition, the nature of the task seemed such that both sexes were quite highly motivated by competing against their own previous performance.

There is no evidence from observations made in the course of the experiment or from the data obtained that the different groups of children interpreted the instructions differently. Great care was taken to ensure that the children were aware of both the speed and accuracy components of the task, and if their performance became erratic they were reminded of the need to aim at the centre of each circle.

During childhood the neuromuscular system is undergoing rapid growth; this growth allows a child to acquire a wide range of skills. However, no amount of training can enable a child to perform to a level for which its sensory, muscular and neural systems are not yet ready. The relative importance of maturation and learning depend not only on the age of the subjects but also on the nature of the skill involved. Sensory-motor skills involve not only input and output mechanisms but also integrative development (Birch & Lefford, 1967). It is also important to remember that non-specific training will occur outside the experimental situation; other motor tasks will give subjects further experience in eye–hand co-ordination. It is interesting to compare the performance of children in this experiment with the normative descriptions provided by Gesell & Ilg (1946). In their behaviour profile of the 6-yr.-old they state: 'He is reported to be awkward in performing fine motor tasks...His pencil grasp is less awkward than at 5½ years, but his performance is laborious...He changes his grasp on the pencil.' By contrast they report of the 8-yr.-old: 'There is an increase of speed and smoothness in fine motor performance. Approach and grasp are rapid, smooth and even graceful; release is with sure abandon.' These changes reported by Gesell & Ilg are to some degree paralleled by the findings reported above and clearly reflect developmental changes in several systems, not only in muscular control but in perceptual mechanisms and, perhaps more important, in central information processing systems. Increases in conduction velocity in peripheral nerves with increasing age, reported by Hazard (1936) and Gamstorp (1963), along with the changes in technique which have been described, are probably responsible for the increase in speed of dotting between the 6- and 10-yr.-old groups.

The literature on sex differences in motor performance is equivocal. Goodenough (1935*b*) and Jones (1937) found no real differences in reaction time between boys and girls over the age range 4–20. Hicks (1930) found that boys were better than girls at a moving-target task, though the girls made greater gains with practice. Jones (1949) reported a slight superiority in adolescent girls for tasks involving the finer motor skills. Kohen-Raz (1965), testing children aged 7–14 on a target test, found that girls had a 'generally lower achievement in hitting the target'. This conclusion, however, seems to reflect sex-role differences rather than basic differences in motor skills.

Davol *et al.* (1965) found no evidence of a sex difference between several age-groups of children on a pursuit rotor. However, in a tracking task such as that used by these authors speed and accuracy cannot be disentangled and any sex difference on the speed component may therefore have been masked.

It is probable that the relative superiority of either sex depends upon the precise nature of the task. When the task is simple and not contaminated by sets from sex roles, then a superiority in favour of girls may reflect more rapid neural and skeletal maturation. Tanner (1961) offers evidence showing that boys remain about 20 per cent behind girls in skeletal age from birth to maturity. Similarly cerebral inhibition, measured in terms of associated movements, develops more rapidly in girls (Connolly & Stratton, 1968).

In contrast to the results for speed of performance the analysis performed on the accuracy scores shows no significant age or sex effects. The fact that accuracy does not improve with age would seem to indicate, at least, that the youngest children had achieved that level of muscular control enabling them to perform efficiently this particular type of motor task. On the other hand, the fact that accuracy decreases with practice, while speed increases, indicates that the two components are compensatory, and this suggests that the older children may have sacrificed some potential extra accuracy in favour of the extra speed which they actually achieved. It is not therefore possible to conclude that 10-yr.-old children could not have been more accurate than 6-yr.-olds.

The changes in scatter of the dots with increasing age does suggest one important development which takes place over the age-span covered by this experiment. This is the ability to perform the task, essentially the alteration in direction, as a smooth and unitary whole rather than as two apparently discrete components. Table 5 suggests that this may appear earlier in boys than in girls, although the sample is so small that the observed difference is not statistically significant. Even if there is a superiority in technique with boys, it still appears that girls respond faster, which adds support to the contention that the sex difference is not an artifact of the task.

The manual skills are not only our chief examples of fine motor ability but they are also characteristically familiar functions which have played an essential and distinctive role in man's evolution and cultural progress. Much remains to be discovered about the processes involved in the development of these important skills.

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RECENT TRENDS IN PSYCHOLINGUISTICS: A CRITICAL NOTICE

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The Genesis of Language—a Psycholinguistic Approach. Edited by FRANK SMITH and GEORGE A. MILLER. Boston and London: M.I.T. Press. 1966. Pp. xii + 400. 80*s.*

Psycholinguistic Papers. Edited by J. LYONS and R. J. WALES. Edinburgh University Press. 1966. Pp. 243. 42*s.*

I

These books are concerned with the description and theory of language acquisition and language behaviour in the native speaker. This is sometimes called psycholinguistics. *The Genesis of Language* contains the 13 precirculated papers with comments and edited discussion, plus a summarizing introduction and final overview, of a conference on 'Language Development in Children' in Virginia in 1965. *Psycholinguistic Papers* contains the five precirculated papers with comments and edited discussion of the conference on psycholinguistics at Edinburgh University in 1966.

The Virginia conference was concerned with child language, language acquisition being taken to be of fundamental importance for any general explanation of language behaviour. The Edinburgh conference was concerned both with language acquisition and with the general problem of understanding and producing spoken language. The papers in *The Genesis of Language* fall roughly into three groups: theoretical, empirical/descriptive and comparative/biological. The theoretical papers by McNeill, Fodor and Slobin propose models for child language acquisition. The descriptive papers give information about systematic observations on child language. Two of these deal with normal development; Ruth Weir on problems of relating intonation to syntax in very young children, and M. Templin on pronunciation and other language skills of school-children; and two papers, by D. B. Fry and Ira J. Hirsch, discuss the language problems of deaf children, and how to overcome them.

The comparative/biological papers are 'concerned less with the specific problems of human language than with the general process of communication and their biological foundation'. Lenneberg emphasizes the importance in language development of physiological maturation as against learning. Chase discusses purposes and forms of communication in various species. Kalmus spoke as an ethologist on prelinguistic baby communication, as he was invited to do, but he also found it necessary to instruct the conference participants about some elementary concepts in genetics. Finally, there is a fascinating paper on trying to teach chimpanzees to speak. The authors, Premack & Swartz, have not succeeded yet, but they have done some hard thinking about the processes of communication and the requirements of a simplified artificial language.

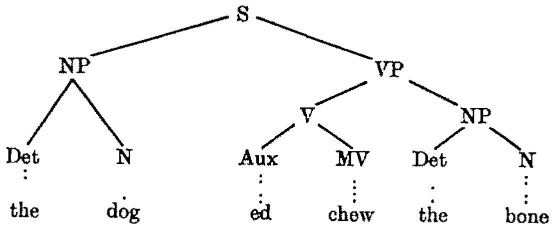
In *Psycholinguistic Papers*, two of the five contributions concern child language; Klima & Bellugi propose a grammatical description for negatives and questions in children, and McNeill proposes a considerably amended model for child language acquisition. The three remaining papers postulate performance models for the adult language user; J. P. Thorne argues that perceiving and understanding sentences is one indivisible process, Wales & Marshall survey the experimental literature exhaustively and suggest a scheme for sentence recognition and production, and Fodor & Garret conclude, after reviewing contradictory experimental results, that the relationship between grammatical rules and the rules of language behaviour can only be stated in very abstract terms.

II

The authors of the theoretical papers and most of the other authors, both in *The Genesis of Language* and in *Psycholinguistic Papers*, owe allegiance to the linguistic and epistemological views of Noam Chomsky. It is impossible to set these out clearly and correctly within the space of a review—it is difficult enough under any circumstances—but some attempt must be made. Without the background, the arguments are meaningless.

According to Chomsky the job of a linguist is to establish a generative grammar; that is, to formulate the rules by which well-formed sentences and their descriptions are generated. *Well-formed sentences* are theoretical constructs, not the utterances that people produce, and *generated* does not mean produced by people. Well-formed sentences are the end-product of an algorithmic process whereby abstract grammatical entities or categories are subjected to permissible and explicit operation or rules. A proportion of the utterances in any natural language (like English, Russian, Cambodian, etc.) correspond to well-formed sentences; these are commonly referred to as the sentences of that language.

There are two kinds of grammatical rules: phrase structure rules (also called formation rules) and transformational rules. Phrase structure rules provide for the hierarchical segmentation of the underlying base of a sentence and for the identification of the segments or constituents. They can be represented as a tree diagram, for instance:



(S, sentence; NP, noun phrase; Det, determiner; N, noun; VP, verb phrase; Aux, auxiliary; MV, main verb.)

The first rule which is represented in this simplified phrase marker or tree is

$$S \rightarrow NP + VP,$$

which reads: 'Sentence; rewrite; noun phrase plus verb phrase.' The lower line of the tree:

the dog ed chew the bone

is the base string which underlies a number of derived sentences. Sentences are derived from bases by the aid of the second sort of grammatical rules, the transformational rules. From the base string:

the dog ed chew the bone

a simple re-ordering rule generates the sentence

the dog chewed the bone.

More complex transformational rules are required to generate from the same base a sentence which has undergone a passive transformation:

the bone was chewed by the dog.

Both sentences are generated from the same base. The basic meaningful relations between the agent and the action and the object of the action remain unchanged. All sentences which share the same base have the same deep structure. The phrase marker gives the deep structure. Transformations do not alter the meaningful relations established in the deep structure.

The constituents of the phrase structure: NP, VP, Det, N, etc., and the relations between them: subject/predicate, modifier/noun, etc., constitute deep grammar. The transformational rules are surface rules and the derived sentences are surface sentences, their structure is surface structure. The base string is arrived at by inserting at the bottom of the phrase marker the appropriate words or items from the lexicon. The base string provides a particularized summary of the deep structure underlying a set of well-formed sentences.

These grammatical postulates are not verified empirically. They are validated by appealing to the native speaker's intuitive judgement of the essential meaning relations which are formalized in the phrase marker. Actual language events are faulty, at best they reflect surface structure only. Generalizations from observation of raw language data do not and cannot validate basic postulates. Chomsky is a self-proclaimed rationalist, not an empiricist (the title of his last published book is *Cartesian Linguistics*). His basic postulates are either self-evident, or they are nothing. The correct grammar is arrived at by the correct application of the correct rules to the correct entities, as in mathematics. However, it is also necessary to demonstrate the relevance of the theory to natural language. This is where language data comes in. The method is illustrative. Crucial examples are adduced to show that the theory is compatible with selected instances.

This rationalist orientation, though familiar enough among philosophers, has not been common among scientists in the last century or two. But Chomsky and his disciples cannot be fully understood unless their rationalist philosophy is appreciated.

Chomsky further insists on the importance of the distinction between descriptive adequacy and explanatory adequacy. Descriptive adequacy is his term for the usual scientific aim of arriving at a theory which accounts exhaustively and economically for the data. A grammar is descriptively adequate if it accounts explicitly for all the grammatical sentences of a language, i.e. if it describes a language. This is what all grammarians try to do, and what nobody so far has achieved. Chomsky does not pretend that he has either, but he thinks that this is not sufficient. Since there could be more than one such descriptively adequate grammar one must have ways of choosing the best among them. His criterion is that the grammar should have explanatory adequacy.

It is not easy to sort out what this means. The first simple answer is that one must ask not only for the 'how' (descriptive) but for the 'why' (explanatory). This is illuminating only in a vague sort of way.

The ultimate answer seems to be that the best grammar is the one which is established according to the correct linguistic theory. This is a philosophical requirement, not a scientific one. The underlying assumption is, of course, that the correct theory is some version of the phrase structure/transformational theory.

The competence/performance distinction is again only intelligible from the rationalist point of view. 'We thus make a fundamental distinction between *competence* (the speaker-hearer's knowledge of his own language) and *performance* (the actual use of language in concrete situations)' (Chomsky, 1965, p. 4).

The concept of competence covers both the grammar and the native speaker's knowledge of the rules of the grammar. It is the last part which is difficult, because it turns out that 'knowledge' does not imply action or behaviour of any sort: neither the ability to act on the knowledge, since 'the actual use of language' is in the domain of performance; nor the awareness or ability to formulate the grammatical rules. The knowledge is 'tacit' or 'implicit'; in fact, it must be understood as knowledge in an epistemological rather than in any psychological sense.

A theory of performance is necessary to account for language behaviour. Competence will be a basic component in performance, but there will be others, such as intelligence, attention, memory span, etc.

Performance accounts for the fit, and for the lack of fit, between well-formed sentences generated by the grammar, and the actual utterances produced and received by people who speak some natural language, e.g. English.

'The problem for the linguist, as well as for the child learning the language, is to determine from the data of performance the underlying system of rules that has been mastered by the speaker-hearer, and that he puts to use in actual performance' (Chomsky, 1965, p. 4).

That is to say, the child acquires language by proceeding like a linguist, to discover the underlying system of grammatical rules. It is Chomsky's contention that it is impossible to discover the correct rules from the faulty utterances—the data of performance—that the child is exposed to (or the linguist, for the matter). The general epistemological point is that one cannot discover a principle or a classification without having a prior hypothesis of its existence. This is a point repeatedly taken up, for instance, by Fodor (in *The Genesis of Language*) and by G. A. Miller (Miller & Isard, 1963).

Since the child is so astonishingly successful in acquiring language, he must have an innate *faculté de langage*, a Language Acquisition Device (LAD) which has built into it a set of specifications for correct grammars, and a testing device which allows the child to discover from the scrappy performance data which particular grammar, out of the small set of correct grammars, obtains for whichever natural language he is exposed to.

The LAD is innate and common to all members of the species. All children learn their particular language. They are able to do this because their LAD's postulate the universals; the rules and constituents, which underly all languages. The problem of child language acquisition resolves into the quest for universals. Even further 'the main task of linguists must be to develop an account of universals' (Chomsky, 1965, p. 28).

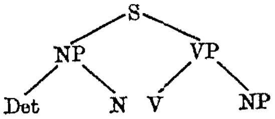
III

McNeill and Fodor, Fodor & Garret, and Slobin contribute papers on child language. They all accept Chomsky's formation/transformation rule grammar. They also (with the possible exception of Slobin) accept Chomsky's psychological notions. They accept that language acquisition is a process whereby the individual arrives at 'a generative grammar that expresses his knowledge of his grammar' (Chomsky, 1965, p. 8), and that this process is not a matter of learning from experience, but of applying the innately known language universals to the local language data. They accept that any S–R type theory, relying on parameters of frequency and reinforcement and on mechanisms like mediation, is not merely inadequate but irrelevant. They accept that their job is to specify the language universals the child is equipped with, and to establish the grammatical rules that the child adopts on its way to adult competence.

McNeill's two papers illustrate perfectly the inherent difficulties of this approach. In the paper 'Developmental Linguistics' in *The Genesis of Language*, McNeill puts forward his first formulation of the innate linguistic universals. These are: (1) the basic grammatical relations, i.e. the symbols and rules represented by the phrase marker (cf. p. 316); and (2) a 'hierarchical classificatory system'.

Grammar begins with two-word utterances. McNeill follows Braine (1963), Ervin (1964), Bellugi & Brown (1964) in postulating for his mark 1 model two primitive grammatical classes: the pivot class and the open class. The words in the pivot class are few and are used often, always accompanied by a word from the open class. There are many more words in the open class, but each word is used less often. Adam, the boy studied by Brown, Bellugi *et al.*, had, at stage 1, pivot words like *here*, *my*, *allgone*, etc., and in the open class he had: *Adam*, *Mummy*, *coat*, *baby*, *show*, etc.

Adam's data are compatible with the postulate that the adult grammatical classes 'are merely removed from the pivot class like banana peel' (*The Genesis of Language*, p. 28), for by stage 2 in his development Adam had removed articles (a, the) and demonstratives (this, that) from the undifferentiated pivot class, and had established these as separate classes. By stage 3 he had done the same with adjectives and possessives. McNeill's universals, checked against Adam, are pivots and open class words, plus the constituents of the adult phrase marker, which the child reaches by developing the left side (the NP side), and the right side (the VP side) separately and then joining them up when his intellectual capacity permits. But in *Psycholinguistic Papers* McNeill has an amended set of proposals for universals. By now he has been persuaded that pivots and open class words are not deep grammar constituents, only the surface manifestations of something deeper. He has given up the notion that adult word classes are peeled off the pivot class. Because now he has looked at data from a Japanese child, and it appears among other things that she has adjectives in her open class, as well as in her pivot class—which makes the hierarchical bananapeeling rather difficult. The universals now consist simply of the constituents and relations represented by the original phrase marker:



This brings us back to the basic implausibility that all utterances, child as well as adult, relate, insofar as they are lawful, to one single sentence type. Also it is now less than clear how the classes develop, and what the hierarchical chronological stages are.

The universal postulates that are compatible with the data from child number one are not compatible with child number two. For two children we get two universal models. And for three children?

IV

Large sections in McNeill's two papers, in Fodor, in Lenneberg, in Fodor & Garret and in Wales & Marshall are devoted to ridiculing and otherwise opposing the supposition that orthodox learning theory has any validity or bearing on language behaviour. The basic point is Chomsky's: unless you have an *a priori* hypothesis, you cannot make anything of contingent data, either as a child or as a linguist.

McNeill's main supporting argument seems to be that, since children utter base strings and adults talk in sentences, children cannot have learnt their utterances by imitating adult language. Therefore their utterances must be the result of in-built language universals.

Fodor agrees with McNeill's conclusion, but rejects his argument. Fodor denies that anybody, child or adult, utters base strings. He quotes a few Chomsky examples to show that sentences are generated from base strings, not from other sentences. He assumes that the processes of utterance are parallel to the generating processes. Since children produce sentence-like utterances, since these must derive from base strings, which are never uttered, children must have access to base strings innately. But, if children's early utterances do at some stage correspond to base strings, as McNeill believes, what then?

Lenneberg is a clinician, interested in linguistics, who publishes in journals read by linguists and who is accorded enormous respect by them. Whereas McNeill, Fodor and others elaborate on Chomsky's *a priori* hypothesis argument, Lenneberg takes up Chomsky's (1959) argument by analogy (advanced in the savage and largely irrelevant review of Skinner's *Verbal Behaviour*): if imprinting behaviour is largely innately determined, why not language behaviour? Lenneberg substitutes motor development for imprinting, which at least has the advantage that it pertains to human beings. He states that language and motor development are largely parallel, even in the severely mentally retarded. 'The preservation of synchrony between motor and speech or language milestones in cases of general retardation is, I believe, the most cogent evidence that language acquisition is regulated by maturational phenomena' (*The Genesis of Language*, pp. 224–225). Setting aside the logic of this proposition, it is vulnerable to any finding that motor and language behaviour develop at different rates in a significant proportion of cases.

Lenneberg also tries to argue that language does not develop in response to 'need', and is not significantly affected by amount of experience, i.e. that it is like instinctive behaviour. One of his arguments about need runs somewhat like this: A healthy deaf 2-yr.-old communicates his desires, needs and even opinions perfectly satisfactorily by pantomime and other non-verbal techniques. So why should children who hear bother to undertake the complicated task of learning to communicate by language, since he does not need language? Because, according to Lenneberg, the task of language acquisition is not complicated, 'it comes naturally' (*The Genesis of Language*, p. 232). Lenneberg's understanding of the concept of 'need' and his willingness to accept 'natural' as an explanation of behaviour seem equally naive.

There are two positions one may adopt on the innateness of the ability to learn and use language. The moderate or 'weak' position is common ground; though animals communicate, only human beings communicate by language. Language is human specific. There must be something that human infants have that, for instance, chimpanzee infants do not have. The extreme or strong position is the standard Chomsky–McNeill–Fodor line; the child is born with the basic grammatical relations and hypotheses in its head. As Colin Fraser puts it in his comments on McNeill's paper, 'The Creation of Language by Children': 'Metaphorically speaking, a child is now born with a copy of *Aspects of the Theory of Grammar* tucked away somewhere inside' (*Psycholinguistic Papers*, p. 116).

Slobin, in his generous comments on McNeill's paper and in his own paper, accepts the Chomsky grammar, the competence/performance notions and the need for a model for a language acquisition device, but departs from the strong position in several respects. He finds it difficult to believe that the child is pre-programmed (*a*) with advance knowledge of the contents of the phrase structure tree (though he is inclined to allow the P and O classes), and (*b*) with specific hypotheses about animate/inanimate, past/present, etc. He suggests instead that the pre-programming may consist of the ability to learn certain types of categories, not specified in advance. He sees language learning as a special case of concept learning (not as the product of a *faculté de langage*) and suggests that it is through language experience that the child's attention is drawn to the possibility of dividing nouns on the basis of animation, verbs on the basis of duration, etc.

It is natural to suppose that Slobin's independence is in large part due to his intimate knowledge of the Russian literature on child language, which is much more extensive than the English.

Slobin makes another very important point when he criticizes McNeill and Fodor for failing to take account of meaning; the relationships between language and all non-language phenomena. Slobin believes that meaning is essential to account for the chronological stages of language mastery. Russian is a language with many morphological classes, i.e. classes of endings suffixed to noun, verb and adjective stems. The observations of the Russian investigators suggest that the rate of mastery of the various morphological classes is related to meaning. (i) Suffixes which have clearly concrete reference come first, e.g. the proper endings for numbers, the diminutives of nouns. (ii) At a later stage comes command of endings governed by systemic or dimensional relations, e.g. case and tense suffixes. (iii) The last stage is mastery of grammatical gender, which has no meaning at all.

With access to innumerable observations, Slobin is able to weight Lenneberg's claim that amount of language experience has little effect on language development against the practical concern of Russian child-language investigators with language enrichment programmes for pre-school children reared in state nurseries. He quotes Lyamina, writing in 1958: 'Insufficiencies of group upbringing have an especially unfavourable influence on the development on speech in children.'

The theoretical preoccupation of *The Genesis of Language* is child language, the predominant preoccupation of *Psycholinguistic Papers* is adult language performance, in the light of the competence/performance dichotomy. The categories and rules of the grammar specify the native speaker's competence, his unformulated, non-active knowledge of his language. A performance model is required to explain how he *uses* language. All performance hypotheses have their source in competence notions.

The first suggestions for performance models assumed that performance categories and rules mirrored competence categories and rules; except for errors, omissions and other limitations. Miller (1962) gives the clearest account of this stage. At that time the grammar insisted on the importance of the sequential ordering of the application of the transformational rules, so the psycholinguists tried to show that subjects needed more time to recognize sentences which were the result of later transformations than those which were the result of earlier transformations. The first results were promising, and this was taken to indicate the psychological reality of transformational rules on the one hand, and on the other to confirm the claim that transformational rules were part of an explanatory grammar.

However, later experimental results did not confirm the earlier ones and, which was more important, Chomsky changed his grammar (it is the revised version I have tried to sketch at the beginning of this review). The proposals in *Psycholinguistic Papers* are representative of the second generation of performance models. They differ from the first generation in two ways. Firstly, they incorporate the surface structure/deep structure dichotomy, which had not been formulated for the first grammar. Secondly, all the contributors (Thorne, Wales & Marshall, and Fodor & Garret) explicitly renounce any claim that performance rules bear any direct or necessary relation to generative rules. What they do claim differs slightly from author to author. Thorne affirms his belief that linguistic categories like *phrase* and *word* have psychological reality. By this he means that these linguistic units are also units of language behaviour, that they enter into the perception and production of sentences.

Similarly Wales & Marshall believe in the psychological reality of the phrase marker, and Fodor & Garret in the reality of the structural description. That is to say, both pairs believe that deep structure enters into performance.

What about the surface structure: is that real?

For Thorne the question would not seem to arise, since he maintains that hearing and understanding a sentence is one single process, like the one-stage structuring of perceptual Gestalts. The implication must be that surface and deep structure is somehow fused in performance.

But Wales & Marshall and Fodor & Garret seem to believe that surface structure enters into performance. In the Wales & Marshall scheme, there is a component called the 'preliminary surface structure analyser', while Fodor & Garret have a device they call the 'recognizer'. The input to both is sentences; and the structure of sentences is surface structure.

The next question is, how does surface structure get converted into deep structure (and vice versa in production), since the psychological reality of transformational rules is no longer postulated?

Fodor & Garret offer the suggestion that the recognition routine might include a system of rules that are arrived at 'by systematic distortion of the rules in the grammar' (*Psycholinguistic Papers*, p. 140). This can only mean that the performance rules are like the generative rules, except that in some ways they are not. Wales & Marshall are even less explicit on this point, though they do suggest that the performance rules must be able to process 'relational notions'—presumably as distinct from conjunctive and disjunctive notions.

The rules the contributors no longer believe in for performance are the transformational rules. They do not state whether they believe in the formation rules or not. Perhaps they do, since they believe that deep structure enters into performance, and it is the formation rules which define the relationships between the deep structure constituents.

The relationship between competence and performance is left obscure. Fodor & Bever think it is 'less obviously sound' to suggest 'that a performance model ought to consist of a model of linguistic competence (a grammar) plus some further component or components at present unknown; jointly these components are somehow to issue in the utterance or understanding of sentences' (*Psycholinguistic Papers*, p. 138). Precisely so. Yet this seems to be what Wales & Marshall suggest. They find it 'already clear' (*a*) 'that competence is neutral with reference to many performance problems', and (*b*) 'that it is in some sense true that performance incorporates competence' (*Psycholinguistic Papers*, p. 54).

VI

This review has concerned itself in some detail with the theoretical models proposed for child language and for language performance, all within the same linguistic and epistemological framework. Model-making is fascinating and necessary. The contributors bring to their task a high degree of linguistic sophistication, a close knowledge of the relevant literature, a determination to render explicit all assumptions and all procedural steps, and great skill in conducting the debate on their own terms. Their papers are enormously informative and stimulating, though not necessarily easy to read. They represent a very vigorous and productive movement. Having been exposed to their work and their ideas, it is impossible to remain complacent about any notions one may have had about language or grammar or language learning or language use.

The model-makers demand of themselves that their models should be correct; pragmatic approximations they do not find interesting. The features of their models are either self-evident, or they are deductions from self-evident propositions. But every proposed model differs more or less importantly from the rest, so they cannot all be correct.

There are, I believe, several reasons for the ephemeral nature of such models. In the first place, any psycholinguistic model which is closely tied to any particular grammar is doomed to obsolescence in a very short time, because the rate of change in linguistics at the moment is such that any theoretical or descriptive work is out of date by the time it appears in print. The second reason is that the models are constructed in such a way that operational criteria cannot begin to apply. If Fodor & Garret postulate that in performance the conversion from surface to deep structure is accomplished in one component, while Wales & Marshall require two components for roughly the same purpose, what conceivable operation could help one to decide between these two alternatives—or the next one when that appears?

Finally, there is plain ignorance, which is nobody's fault. In an interdisciplinary field nobody can know everything there is to know, and in child language nobody knows very much. All the same, it is ignorance which allows Chomsky to think that language behaviour has much in common with imprinting behaviour, and equally it is ignorance which prompts psychologists to make statements about language they do not know much about. Some of the statements made at the Edinburgh Psycholinguistic Conference about word order in Russian upset at least one Russian-speaking professor of linguistics. We do not know nearly enough about what very young children actually say, though we are beginning to learn a little, mainly about children who are in the process of acquiring English and Russian.

However, the theories of those who describe child language seem to matter surprisingly little. The Russian studies are tremendously informative and valuable, regardless of whether one accepts the Pavlov–Vygotsky–Luria framework in which they are presented. Braine, a psychologist at the Walter Reed Hospital, Washington, mainly interested in language pathology; Brown and Bellugi at Harvard, inevitably influenced by the transformationalists, and Susan Ervin in California, experienced in the more traditional types of verbal learning experiments—all conducted intensive studies in child language and all three concluded quite independently that the words children use in their earliest two-word utterances can be classified into pivots and open class words. Conversely, the utterances of Adam, collected by the Brown–Bellugi group, have furnished the material for quite a number of Ph.D. theses (Slobin's and Cazden's among them), not all necessarily written from the same theoretical point of view.

VII

Sweeping statements about child language in general are premature in our present state of ignorance, let alone claims of universality. The most useful work that can be done in the field of child language at the moment is to observe and record children's utterances in their contexts, and to organize and present these findings in a way which is linguistically sound, but not too closely tied to any particular school, i.e. according to principles which most linguists would accept. The longitudinal study of child language is a very time-consuming affair, requiring roughly one researcher for each child, and this researcher will not be able to process more than something like four hours of recording per month, often less. Only by presenting the data so that the information can be pooled, can one hope to avoid the grossest kinds of sampling errors.

Descriptive papers, such as Bellugi & Klima's in *Psycholinguistic Papers* and Ruth Weir's in *The Genesis of Language*, are at least as important as the theoretical ones. But they are not controversial so they do not occupy much space in a review.

These two books will be invaluable to anybody interested in the field of psycholinguistics. Between them they reflect the recent theoretical positions and they provide a good deal of information about relevant work.

The Genesis of Language gives the broader picture and the more reasoned presentation; *Psycholinguistic Papers* offers the most recent (published) developments and also some very acute and valuable criticisms by the commentators, particularly those by Fraser, Sutherland and Cohen. In addition, *The Genesis of Language* has Slobin's extensive survey of the Russian literature, and *Psycholinguistic Papers* has a very useful bibliography of mainly American work, compiled chiefly by Wales and Marshall.

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PUBLICATIONS REVIEWED

Principles of Behavioral Analysis. By J. R. MILLENSON. New York and London: Macmillan. 1967. Pp. xxii + 488. 63*s*.

This introductory text proceeds on the assumption that students need a rigorous, systematic and graduated text, written from a simple theoretical viewpoint (operant conditioning) and clearly based on experimental data. It omits many of the topics of the introductory text, concentrating on an intensive, laboratory-orientated discussion of learning processes, motivation and emotion. The student is presented with a clear-cut, unambiguous vocabulary, a set of S–R symbols for analysing behaviour, statistical concepts and techniques for evaluating laboratory data and a consistent theoretical viewpoint. The relevance of the animal laboratory (where a great deal of the text is orientated) to the human situation is drawn out, and the power of this model in the explanation of behaviour is made clear.

This is a highly unusual and well thought-out approach to the question of introductory texts. If one accepts the principles of learning theory (especially à la Skinner) and if this text is the harbinger of many more of a similar character, it can be said that the teaching of psychology is beginning to take the actual findings of psychology seriously as far as higher education is concerned. The author has taught in American, Canadian and British universities. His text lacks the usual weaknesses of introductory American texts—the style is concise, clear and relatively free of stylistic jargon. The book is enlivened, but not oppressively so, with tables, line-drawings and diagrams. It can be highly recommended.

JOHN MCLEISH

Conditioning and Instrumental Learning. By EDWARD L. WALKER. Belmont, Calif.: Brooks/Cole. 1967. Pp. xii + 161. 14*s*.

This book is part of the series 'Basic Concepts in Psychology', a fragmented textbook of the type also developed by other publishing houses of late.

The scope of Walker's book is narrow, for skill-learning and human complex learning are dealt with elsewhere in the series. It is organized 'in order of the amount of variability involved' in various experimental situations. Minimal variability is represented by the behaviour attributed to instinct, imprinting and early experience. Maximal variability is supposedly found in work on imitative behaviour in animals and the 'modelling' of children (Bandura). Between these two extremes, the bulk of the book is devoted to classical and instrumental conditioning. These are briefly identified, and many problems of interpretation are mentioned. However, not a great deal is to be learned from the introduction of many of these problems. Nor can it be otherwise when the contiguity/reinforcement issue is dealt with in 34 lines (conclusion: a 'moot' point), the problems of punishment are reduced to the dubious observation that one sometimes finds paradoxical results ('no simple solution to the punishment paradox emerges'), and the problem of whether learning is a unitary process is handled in two pages (conclusion: this problem 'must remain unresolved'). A chapter on operant conditioning may be the most adequate for first-year students, for this consists mainly of methods and results rather than arguments.

The final chapter introduces mathematical models. This can certainly be justified, but one sentence spills the beans. Walker displays a computer program; but 'the reader is not expected to understand it' unless he happens to know its language. Then why devote a whole page to it? Perhaps because it may *look* impressive to the reader. Such a suggestion must be unfair to Walker; however, this reviewer feels that such a philosophy is to be found throughout the book. If no reasonable attempt is made to answer them, or to relate them to wider issues, why bother to introduce controversies? Perhaps because it shows our first-year student that we can only hint at the complexities of the study he has chosen. Such an approach may be sound. However, if one disagrees with this answer to the problem of introductory texts, one may exercise the right to select the relevant part of *another* fragmented series. At least, that is one advantage of this system!

DEREK BLACKMAN

Attitude, Ego-Involvement and Change. Edited by CAROLYN W. SHERIF and MUZAFER SHERIF. New York and London: John Wiley. 1967. Pp. x+316. 75*s*.

In 1965 Sherif, Sherif and Nebergall presented their social judgement/involvement approach in *Attitude and Attitude Change.* The title of the present book, and the names of its editors, may suggest that it is a continuation volume. However, the assumption would be rather misleading. The task of the symposium from which the book came was defined by Sherif in a vague and general manner: 'a stock-taking of the state of theory, method and research on attitude and attitude change'. As Sherif also points out, many topics, theories and concerns in this area are omitted entirely. Fifteen assorted psychologists and sociologists talk about their recent work and thinking, and, as with most such symposia, only a few of the contributions are useful or provocative. There is little integration and insufficient mutual relevance among the offerings.

There *is* some concern with the social judgement/involvement approach. The Sherifs contribute a good summary of the operational procedures and the assumptions behind them, and draw the implications for attitude change. They are able to specify the considerations necessary for accurate prediction of attitude change, to show that relevant research falls into a pattern consistent with these considerations, and to contrast their own approach with that of dissonance theorists. Diab argues for a synthesis of the latitude approach with the semantic differential technique. A further paper, by Whittaker, closely examines the issue of communication discrepancy and attitude change, concluding from research results that knowledge of latitudes of acceptance and rejection is indispensable for predicting or effecting change.

The latitude approach, however, seems in this book to be part of a more general theme. Other contributors (Campbell, Rosenberg, Harvey, Triandis) in their different ways embrace a kind of phenomenological emphasis, indicating the necessity for careful measurement of individual differences in complex attitudinal structure—as opposed to measurement requiring endorsement of a single point on a single continuum.

Another broad theme is a plea for realism in attitude research, for an avoidance, as Sherif says, of 'laboratory experiments applied to piddling little problems that cannot be considered significant even by the greatest stretch of the imagination'. Weick insists that there ought to be *no* limitations on the experimental method in the study of attitudes, but that this requires a more imaginative use of the experiment, and he makes a number of provocative suggestions towards this end. Koslin is more equivocal about experimentation, and finally plumps for the testing of theory both outside and inside the laboratory. Several writers argue the importance of studying reference groups and other social influences, and the necessity to find out what people do as well as what they say. The few passing swipes at dissonance theory serve to remind the reader that no dissonance theorists contributed to this symposium's rather perfunctory stocktaking. You cannot even look up 'dissonance' in the subject index, because, despite the high price, there isn't one.

NEIL WARREN

Theories of Attitude Change. By CHESTER A. INSKO. New York: Appleton-Century-Crofts. 1967. Pp. viii+374. $7.50.

The author makes it clear in his 'declaration of intent' that his concern is not with the general reader but with the specialist in the field of attitude change. The result is a comprehensive handbook which should make its way to the shelves of many social psychologists, who will find it a most useful reference.

In his introductory chapter, Insko considers various problems associated with research into attitudes: the concept itself, the various experimental designs for measuring change and the possible distortions in experiments on attitude change. He draws attention to the important work of Orne and others on demand characteristics (the cues which convey the purpose of the experiment to the subject) and of Rosenthal on the related problem of experimenter bias. The succeeding 12 chapters are devoted to the 15 theories, and each chapter has the same format; an account of the theory is given, the relevant research is summarized and, finally, both theory and research are evaluated. The length of the chapters varies widely, much space being allotted to Festinger's cognitive dissonance theory and to Hovland, Janis and Kelley's reinforcement theory, while only a few pages suffice for those of Sarnoff and McGuire.

Insko has made a thorough job of summarizing and evaluating the theories. Particularly successful is his chapter on dissonance theory, where he weighs up, in the light of recent developments, the stringent criticisms made by Chapanis and Chapanis, and demonstrates how accurately dissonance theory is able to predict changes in attitudes following decisions or actions. His treatment of reinforcement and balance theories is equally effective. However, one must question Insko's decision to include some of the minor theories. Adaptation level theory, for example, has only marginal relevance for social phenomena, while the research by Helson and his associates bears little relationship to the theory. Similarly, the theory put forward by Smith, Bruner and White is hardly concerned with attitude change but with the functions attitudes serve for personality.

In spite of this lack of selectivity, Insko has fulfilled his intention. He has provided us with a comprehensive handbook. His summaries of theory and research are concise yet packed with information; his evaluations of each theory are balanced and insightful. Nevertheless, one is left with a sense of frustration after completing this book. Insko makes virtually no attempt to integrate the diverse theories in his brief final chapter. He does suggest that two important principles—consistency and reinforcement—run through most of the theories. He does not elaborate this interesting notion into a consideration of where the theories overlap, in what ways they conflict, and how contrary predictions could be tested. It is this kind of integration made by, for example, Roger Brown, linking congruity, balance and dissonance theories to the psychology of conflict, that is the kind of integration which is lacking in this book.

Another criticism is a more personal one directed at the whole field of attitude change. Theorizing has become more experimentally based and more sophisticated over the past 15 years. This has meant a great advance over the piecemeal 'fact-finding' exercises of the 1930s and 1940s. But the subject-matter has become less and less 'ego involving' in Sherif's sense. The theories generate experiments which rely on trivial and peripheral attitudes as subject-matter: thus a recent experiment has shown that people become more favourably inclined towards fried grasshoppers after being persuaded to eat them by an unpleasant, rather than a pleasant, authority figure. This may have interesting implications for the theory of cognitive dissonance but how much does it contribute to the social psychology of attitudes?

JAMES H. WHITE

The Impact of Television. Methods and Findings in Program Research. By WILLIAM A. BELSON. London: Crosby Lockwood. 1967. Pp. x+410. 50*s*.

The majority of the studies reported and discussed in this book date from the eight-year period spent by Dr Belson with the Audience Research Department of the B.B.C. A number of the studies have already appeared in the journal or as Audience Research Department reports, but it is convenient to have them collected together in the present volume.

The presentations of empirical studies are arranged under four major headings. The first section deals with work aimed to assist in the planning of television (and radio) programmes, and focuses on such topics as the audience's prior levels of information and existing attitudes. The second section covers the comprehensibility of broadcast materials, and thus deals with the evaluation of actual programme content. The third section of the book is concerned with the effects on knowledge and attitudes of particular television programmes, while the fourth and last major section deals with the broader social effects of television: the impact on interests and initiative, the impact on family life, and the impact on other media activities. Each of these four sections is arranged in a similar fashion: the social and pragmatic rationales lying behind the particular sort of inquiry are outlined in an initial chapter, and this is in each case followed by a second chapter on the appropriate research designs (particularly the author's own 'stable correlate' method) and quantitative techniques. A varying number of chapters in each section then present the results of particular investigations.

While there is no denying the practical utility and social interest of much of the data reported (for example, that on the delayed return of leisure-time pursuits to nearly their former level some 4–6 years after the innovation of television), the most salient feature of the book for any social psychologist will undoubtedly be the lack of any theoretical deductions from the mass of available data, and the apparent unwillingness to relate the findings presented to any extant body of theoretical propositions. The contention can be illustrated by reference to chapter 19, which reports on a study of the television series entitled 'The Hurt Mind' (which dealt with various aspects of mental illness). Although some interesting attitudinal changes are reported, no theory of attitude change is invoked to account for these, so that the interest of the study for the social psychologist is minimized. Perhaps one of the secondary reasons for this lack of theoretical analysis lies in the fact that viewers are treated here as a unitary group, so that the necessity of accounting for possible differential effects on different types of individuals is not allowed to emerge.

However, the lack of any theoretical analysis at this and other points in the volume appears to be a matter of principle rather than of oversight or of the unsuitability of the data, for the sole chapter in the fifth section of the book stands as a conscious (and even occasionally aggressive) *apologia* for the mode of research and analysis adopted. Following a balanced and insightful analysis of the sociology of audience research within the television industry in this country at the present time, a call is made for a much increased volume of research into the effects and effectiveness of television. While the social need for the wider availability of more detailed audience research can be admitted, however, the ways in which it is suggested that research could help both the television industry and society at large contain a number of ambiguities. 'Academic' (presumably social psychological) research into the communication process is at one point rejected in favour of the assessment of short-term effects as a direct service to broadcasters, and yet is readmitted as legitimate when the proper research programme of a television institute is sketched in; while a certain *naïveté* is detectable in the implied hope that research in the interests of the 'public welfare' can easily be carried on within the context of what is presented as a predominantly competitive industry.

Dr Belson apparently wishes to interest social scientists in television research, but to do so he may first need to realize that the social psychologist (for example) has other interests besides data-collection techniques, important as these are. But, despite these strictures, it has to be realized that the present volume represents the fruits of one of the few sustained British research efforts in this field.

The bibliography provides a useful listing of empirical studies, though the bias towards industry-supported work is notable. The book is marred by an intolerably large number of misprints.

R. L. BROWN

The Psychology of Anxiety. By EUGENE E. LEVITT. New York: Bobbs-Merrill. 1967. Pp. xiv+223. $1.75.

'Anxiety is the official emotion of our age', wrote Schlesinger in 1948. When one considers the great importance of the concept in contemporary theorizing in the field of personality, it is not surprising that the publishers of the present volume should have seen fit to produce it hard on the heels of Spielberger's *Anxiety and Behavior*. Dr Levitt's book differs from its distinguished predecessor in that it is not a compilation of various experimental approaches and their accompanying points of view. Instead he has exercised some discretion in making a selection of ideas for inclusion. The result is a book which is certainly not comprehensive; for example, no work in perception is quoted. It is also parochial in its coverage of the literature; of 196 references, 190 are to North American sources. Or is it possible that this is a fair balance to strike in assessing the relative weight of American to non-American contributions in this area?

Because of its limited coverage the book does not appear to be as suitable for inclusion in an undergraduate course as its simple, direct style immediately suggests. Indeed, it is hard to see what audience the author had in mind; it will perhaps be most helpful as a preliminary clarification for anyone contemplating research in anxiety.

The first chapters are disappointing: after a preliminary skirmish with semantics the author gives a sketchy and strongly clinically orientated treatment of theories dealing with the origins of anxiety. He then proceeds to a very useful survey of experimental measures of anxiety. Under this heading he summarizes many of the inventories designed to measure anxiety. It is surprising to learn that since the publication of Taylor's Manifest Anxiety Scale in 1953 there have appeared no less than 10 other paper-and-pencil tests, quite apart from variations of the MAS itself. Levitt's short review of these tests is the most useful feature of his book. One might complain of his dismissal of physiological measures in three short paragraphs, although the reasons for this summary treatment became clear at the end of the next chapter, which is devoted to a survey of the physiology of anxiety. After examining various theories of emotion, the brain-stimulation experiments of Olds and others, general arousal theory, and physiological causation in mental illness, the author rather oddly concludes that 'examiners of the physiological have as yet established nothing conclusive'.

A more substantial chapter on anxiety and learning gives a very clear account of the contrasting positions of the Iowa school, who treat anxiety as an energizing drive, and the Yale theorists, who consider it a situationally determined reaction. Levitt then considers applications of the Yerkes–Dodson law to human behaviour and shows that not only does the principle hold for various levels of situationally induced anxiety but also for anxiety-proneness in the Yale sense.

Chapters on the anxious personality and everyday anxieties, followed by a synopsis, conclude the book. There are suggestions for more advanced reading and an index. DEREK FORREST

Developmental Patterns in Reactions to Frustration. By UDAI NARAIN PAREEK. London: Asia Publishing House. 1964. Pp. ix + 182. 25*s*.

It is now over 20 years since Rosenzweig published the adult form of what he called the Picture-Association Study, designed to assess reactions to frustration. In the intervening period the original test has been revised, its title changed to the Picture-Frustration Test, the scoring modified and a Children's Form published. In addition, the past 10 years have seen its extension as an instrument for cross-cultural studies, one of which, using the Children's Form, is reported here.

Dr Pareek, working for the most part alone, has been forced to concentrate on one or two issues, and, as the title suggests, the main part of his study is concerned with developmental trends in the responses given by a sample of Indian children aged 4–13 yr. However, he has availed himself of the opportunity for cross-cultural comparisons by relating his results to those reported in the literature for samples of Japanese and American children.

He is to be congratulated on the care with which his investigation was carried out, and for the commendable caution with which he interprets his data. The shortcomings of his study are, perhaps, more apparent to him than to the reader. One thousand children are a large sample, but he is conscious that, though drawn randomly from the schools in Delhi, they still only represent children in one large city, and that no responses from rural children were obtained. Boys and girls are not always equally represented at all ages, nor are the numbers of children at each age always the same. Indeed, owing to practical difficulties, he could test at years 4 and 5 only about half the numbers of those at later ages. Since all those up to 9 yr. were tested individually, the task must have been time-consuming, and the total sample is impressive.

On the developmental side, Dr Pareek has some interesting results. The responses show a consistent predominance of extrapunitiveness and ego defensiveness at all ages, so far as direction of aggression is concerned, bearing out the results of American studies. There is, however, a decline in extrapunitiveness with age, in contrast to a steady increase in intropunitiveness and impunitiveness. These results may indicate that children perhaps learn to be self-critical (intropunitive) and oblivious of the frustrating experience (impunitive) as they grow older.

In type of reaction, as distinct from direction of aggression, one striking finding is the steady increase throughout the age range of the Group Conformity Rating.

An interesting section compares some results of the Indian group with those obtained in other studies on American and Japanese children. For instance, Japanese children give a higher number of intropunitive responses at ages 6–9; that is, develop feelings of guilt or self-criticism at an earlier age. This may be due to greater authoritarianism in child-rearing than is found in Indian homes or in the Americans. Japanese children are high in obstacle-dominance responses, Indian children very low, suggesting that the frustrating situation is not so frustrating for Indians. This in turn may stem from the general outlook in India (now fast disappearing) that frustration is unavoidable and a consequence of the past. Unlike Japanese, Indian children do not look for a solution from others, a fact which Dr Pareek cautiously relates to the assuming of some family responsibilities at an early age in the Indian home.

It is clear that some interesting differences are present in these three culture groups, and it is to be hoped that Dr Pareek will continue his investigations into such differences. Although the P-F test is far from being a perfect tool, it does appear to show itself as capable of differentiating children of one culture from those of another.

GERTRUDE H. KEIR

The Psychology of Learning and Motivation. Advances in Research and Theory, vol. 1. Edited by KENNETH W. SPENCE and JANET TAYLOR SPENCE. New York and London: Academic Press. 1967. Pp. x + 381. 92*s*.

The series of volumes of which this is the first 'intends to provide experimental psychologists who are actively engaged in research on some specialized topic within the fields of learning and motivation with a vehicle for presenting a systematic integration of the relevant research data—particularly those from their own research programs—on either a purely empirical level or oriented around the evaluation or development of some theory. This serial publication is thus expected to provide a source for papers of a more specialized and original character than review articles now appearing in annual form and of greater flexibility with respect to length and format than is currently found in psychological journals.' Thus Janet Taylor Spence, in her preface; the sad news of Kenneth W. Spence's death, which preceded arrival of the book on the reviewer's table, will not mean, fortunately, that the series will be discontinued. Both the editors must be congratulated on a book which from the points of view of content and production is uniformly excellent, and indeed will prove indispensable to all psychologists interested in the experimental study of the phenomena of learning and motivation. It contains five papers: A. Amsel on partial reinforcement effects on vigour and persistence; E. J. Capaldi on a sequential hypothesis of instrumental learning; H. Fowler on satiation and curiosity; G. Bower on a multicomponent theory of the memory trace; and G. Mandler on organization and memory. The chapters are between 40 and 90 pages in length, and contain ample illustrations and reference material. Such books are notoriously difficult to review—apart from expressing his admiration for the uniformly high level of discussion, and the ingenuity of the experimental work recorded, what can the reviewer say about five highly condensed and carefully argued papers, all written by authorities in their respective subjects? Detailed comments are impossible, and generalized value judgements are impertinent. Perhaps some general comments may be helpful.

Let us note first of all that these contributions (with the predictable exception of Bower's and Mandler's) are neo-Hullian. British psychologists have never taken to Hull's system of behaviour theory, and have thus missed what is by all accounts a salutary experience; their criticisms have tended to be philosophical and rather irrelevant to Hull's attempted contribution. Unfortunately, this has led to British students being deprived of proper knowledge of Hull's system; it must be doubtful if even first-class graduate students could follow these articles easily. The neo-Hullians have followed in Hull's footsteps by accepting his explicit invitation to use those parts of his theory which might be found useful for specific ends; none of them would follow Hull into all the remote corners of his theory—and neither, of course, would he, were he still alive! The way in which Amsel, say, uses Hullian notions and concepts to integrate experimental findings from his research on the motivational effects of frustration, and to go on to design new and ingenious tests of his hypotheses, is wholly admirable; nothing like it is to be found in British literature. In a similar vein, readers are recommended to look at Fowler's attempt to use Hullian notions to explain experimental findings in the field of satiation and curiosity. Philosophical denigrations of system-builders like Hull pale before the demonstration that such systems can actually be used to generate testable predictions, and can also help in unifying large bodies of empirical data. In the second place, note that, in the process of utilizing Hull's heritage, his general theory has of course suffered its inevitable fate; it is breaking up into smaller units dealing with specific problems. As Amsel points out, 'the trend...is toward fragmentation of the Hullian system into separate theoretical treatments—and perhaps very different ones—of the various response attributes'. This again is very much what Hull expected and invited; he was much more aware of the limitations of his system as a *general* body of scientific law than his critics give him credit for.

Hull's theory was orientated along mathematico-physicalistic lines, but it retained links with the biological approach; followers of both approaches could develop ideas latent or explicit in his work. Bower represents a much more clear-cut mathematico-physicalistic type of theorizing which, as he points out, has some similarities to British thinking, e.g. Broadbent, Crossman, Deutsch and MacKay. Here we are concerned with formal structures; Bower explicitly lines himself up with logicians and mathematicians, as opposed to neurochemists or neurophysiologists. Thus his theory deals with such things as pattern-recognition machines, encoders, and other ideas borrowed from information theory. There is a somewhat formidable mathematical treatment of simple memory processes, allied to elegant and simple experiments which exemplify the possible applications of the theory. Only the future can tell whether this type of approach really has much to offer in the study of biological organisms; it will certainly appeal to those who have come to psychology from mathematics or from physics.

Mandler takes up ideas implicit in Katona's work, and explicit in G. A. Miller's notion of 'chunks' of information, and demonstrates interesting consequences of this 'chunking' process for the retention of information. His approach owes little to Hull or to the mathematico-physicists, but might perhaps be called neo-Gestaltist, although he would probably not accept this term. His chapter is certainly the easiest to read and follow, and the one which requires less initial knowledge than any other to comprehend. It also has possibly the greatest immediate relevance to practical issues, as for instance in education.

In praising the quality of the contributions, one cannot forbear to wonder if the second and third volumes in this series can reach a similar standard of excellence. The number of brilliant young psychologists is limited, even in the United States; so is the number of research areas. A second set of five—yes. A third—perhaps. A fourth, fifth, etc.—we will have to wait and see. In any case, this reviewer gives a most hearty welcome to this newcomer to the scene. In case it should be assumed that his glasses are somehow tinted with an entirely rosy hue, perhaps at the end a slight criticism. American scientific writing has inherited all the worst characteristics of German scholarship, and the present authors do not all escape from this generalization. Fowler (to take but one example) sometimes seems to have available to him only two verbs to cover a very wide range indeed—to obtain and to prevail. Just consider all the nouns which prevail: basis (p. 197), novelty (p. 198), limitation (p. 201), problem (p. 209), basis (p. 213), identity (p. 213), issue (p. 214), inference (p. 214), difficulties (p. 217), relationship (p. 217), theory (p. 218), possibility (p. 222), and many more—these were gleaned just by a cursory look through the pages! There is a significant increase in his use of 'prevail' from beginning to end, paralleling the increase in running speed of his rats, and no doubt susceptible to a Hullian interpretation; readers may experience an increasing drive to put down the offending pages. Such writing should certainly come under editorial scrutiny; it is slipshod, pseudo-learned and not directed at making the material easier to understand. Off with his head!

H. J. EYSENCK

Pattern Recognition: Theory, Experiment, Computer Simulation and Dynamic Models of Form Perception and Discovery. Edited by LEONARD UHR. New York: Wiley. 1966. Pp. xii + 393. 68*s*.

This is a book of readings in visual perception. The focus is on pattern recognition, and contributions have been drawn from philosophy, psychology, neurophysiology and computer science. Although the editor's aim was to present an interdisciplinary selection of papers and thus stimulate and inform those specialists who may have only a slight awareness of one another's work, the organization of the book and the editor's comments make it likely that the information flow will be unidirectional rather than multidirectional. That direction is towards computers.

The papers have been selected, not so much with the idea of presenting a cross-section of what is happening currently in the various disciplines, but rather in such a way that the development of ideas may be followed. The book is in five parts: the first gives the broad conceptual framework as discussed by philosophers and psychologists (e.g. Wittgenstein and Craik); the second contains useful reviews of relevant psychological studies by such writers as Vernon and Hake; part 3 presents theoretical developments in model-building, both psychological (Deutsch) and physiological (Marshall and Talbot); part 4 describes some of the recent work in neurophysiology of the visual system and includes papers by Barlow and by Hubel and Wiesel; the final section contains papers on computer models of pattern recognition (e.g. Selfridge, Uhr and Vossler) and a review paper by Uhr.

Reading this book engenders the rather gloomy feeling that the problems of perception were first seen and stated by philosophers; psychologists then juggled with the terminology and perhaps carried out a few somewhat trivial experiments but the problems are now being solved, with no time off for sterile academic debate, by such 'real' scientists as physiologists and computer engineers. This feeling is apparently shared by the editor as it is very much the point of view put forward in his own final review paper. In his notes throughout the book he displays a certain tetchiness towards psychologists, and the inevitability of the progression towards computer models may be viewed as something of a hard sell to those laggards who may yet be persuaded to give up their dilettante ways and get down to real work.

Uhr admits that some psychological models have influenced the computer simulators and cites 'those of Deutsch, Hull, and (in part) of Hebb' as being precisely enough stated to allow of more formal specification. Certainly Uhr has a point when he states that psychological theories have not always been stated with the clarity, precision and consistence which computer programs demand but, after all, the perceptual system is a rather complex affair whose mechanisms are still largely unknown and, by systematically describing the system's overall properties in normal and abnormal conditions, psychologists may still have a useful function to fulfil.

Although the papers have to some extent been chosen to illustrate the editor's argument, the collection is a coherent and stimulating one. If it is also provocative, presumably that too is a good thing.

F. I. M. CRAIK

Multivariate Statistics for Personnel Classification. By PHILLIP J. RULON, DAVID V. TIEDEMAN, MAURICE M. TATSUOKA and CHARLES R. LANGMUIR. New York and London: Wiley. 1967. Pp. 406. 100*s*.

Many books on multivariate statistics must be regarded more as depositories of somewhat incomprehensible facts and theorems rather than sources of information for the ignorant. This rough generalization does not apply here. For the reviewer, this book has been a pleasure to read and to learn from. This is partly because the authors have set out deliberately to teach and partly also because it has been written in a form resembling a programmed text, starting from the simplest case and going on step by step to full complexity.

The authors begin by distinguishing between personnel classification and selection. Since membership of the group determines the former, it is the deviants who must be excluded. This leads to consideration of the contour-score, and from this point the further generalizations are easy. The chapter on topology turns out to be much easier than expected since the word is used to mean 'A mnemonic device based on the association of ideas with places'.

The authors optimistically claim that the book is suitable for those without a background in mathematics and statistics, but this is doubtful. For example, an account of elementary notions concerning frequency distributions includes statements about standard deviations and variances before deviation scores are even mentioned; 'two-tailed' is mentioned (p. 35) without definition. It is unlikely that anyone without a good mathematical background would understand, without reference to a dictionary, the meaning of 'homothetic ellipses'.

But these are all minor criticisms and the over-enthusiastic reviewer can always find minor details to complain about. This book can be heartily recommended to those who wish to learn something about multivariate statistics as well as those who wish to learn about personnel classification. The book is intended primarily for this purpose and not as a reference book. This is confirmed by the fact that the authors have omitted to include an index.

M. HAMILTON

The Human Operator in Complex Systems. Edited by T. W. SINGLETON, R. S. EASTERBY and D. WHITFIELD. London: Taylor & Francis. 1967. Pp. xx+198. 57*s*. 6*d*.

This book is a collection of 21 papers originally presented at a conference sponsored by the Ergonomics Research Society and held at the University of Aston, Birmingham, in 1966. In their introduction the three editors state that the prime object of the conference, and hence of the book, was 'to describe and illustrate the human factors orientated system approach to design for the benefit of research workers and potential users'.

The book is divided into sections dealing with the topics of systems design methods, analytical techniques, allocation of function, task descriptions, training, job aids and maintenance, and applications. As these section headings indicate, the scope of the contributions is particularly broad. The principal merit of the book is that it contains not only papers describing methods of appraising and designing man–machine systems, but also papers which describe actual investigations in which the techniques set out in earlier sections have been applied. These applied papers cover many diverse topics as they range from, for example, the human factors requirements for the development of United States Army material to the design of the operator's task in a tea-blending plant.

In appraising this book there is little doubt that it fulfils the purpose of illustrating the human factors orientated system of approach to design for the benefit of research workers. However, whether the book succeeds in achieving the aim of describing the human factors orientated systems approach to design for the benefit of potential users is less certain. This objection arises not in relation to the quality of papers, since all are relevant to the theme of the book and the authors are all well qualified to write about their subjects. The problem is more one of quantity, for the book's shortcoming is that, as it is the outcome of a conference, it contains the transcripts of papers written originally with one eye on the time. The result is that the papers suffer from a forced compression of content which limits their value and describing function. In the context of the original conference the papers may have also achieved this aim by initiating discussion and eliciting additional illustrative material from their authors. In the book, however, the papers stand on their own merit and, because of their compression, the reader is on occasions forced to look elsewhere for clarification of some of the concepts set out in the papers. Despite this shortcoming, the book contains much of value to both student and practitioner. Both the editors and the Ergonomics Research Society itself are to be congratulated for their efforts in producing this record of what was a most profitable and constructive conference. The result is a worthy addition to the two previous symposia which have been sponsored by the Society.

J. M. ROLFE

Mental Disorder—A Problem with Many Facets. By MAN MOHAN SINGH. London: Pan Books. 1967. Pp. 378. 7*s*. 6*d*.

The aim of this paperback, written specially for the Pan series, is to present the history and present development of psychiatry in terms intelligible to a non-medical reader. In his introduction the author makes clear his belief that 'personal communication' is the most fundamental technique of psychiatry, and one of his main aims in this book is to try to narrow the gap that exists in many areas between the proponents of psychotherapy and those who believe that physical methods such as drugs and ECT have far more to offer in psychiatric therapy.

His book opens with a chapter on the historic roots of psychiatry, ranging from the beliefs of primitive man with his use of skull trephining through to the present day. Modern psychiatrists may well feel proud of their links with Pinel and Esquirol, and prefer to forget certain of their forbears such as Heinroth (1773–1843), who described mental illness as 'the deprivation of freedom of will imposed upon the sinner by God'; or for those less spiritually inclined, the users of the Darwin chair, in which the patients were spun round until they bled from their ears, noses and mouths.

The author then goes on to discuss and to attempt to integrate the various theoretical schools. He is critical of the present common division of neuroses into anxiety states, hysteria, neurotic depression and obsessive–compulsive states, maintaining that the former three syndromes are rarely seen in pure culture. He proposes instead a division of what he describes as psychogenic maladaptive reactions into two main types: affective–dissociative reactions characterized by various combinations of anxiety, depression and dissociation (what many psychiatrists now call the mixed neurotic state), and the obsessive–compulsive reactions.

He gives on the whole a well-balanced account of the aetiology and treatment of psychiatric illness, covering such topics as childhood development, the signs and symptoms of different mental disorders, genetic and social factors, psychological tests used in assessment, the ways in which a patient can be admitted to a psychiatric hospital and the various treatments he may have there as well as the part played in therapy by community care. He pays, however, scant regard to the problems of mental subnormality and the few pages he devotes to this topic make no mention of much of the interesting psychological research at present going on in this field.

Naturally the author's space does not allow him to discuss critically in detail some of the work that he quotes, as one might do in a large textbook, but he is to be congratulated on avoiding the introduction of dogmatic statements for which there is no proof whatsoever, which, owing to lack of space for explanation and expansion, may bedevil short introductions to psychiatry. This book will be particularly useful to students and others who wish to have a quick guide to the wide area covered by psychiatry. There is a short bibliography to provide further reading in specific topics which is quite sound, although I personally deplore the omission, in favour of three earlier works, of what I regard as the two best histories of psychiatry, namely *Historical Development of British Psychiatry* by Denis Leigh and *300 Years of Psychiatry* by Hunter and McAlpine.

BRENDA GRANT

Experiments in Animal Behaviour. By MARGUERITE D. HAINSWORTH. London: Macmillan; New York: St Martin's Press. 1967. Pp. x + 206. 35*s*.

This is a practical guide for setting up simple behaviour experiments suitable for class work and projects by sixth-form pupils and beginning university students. Suggestions and instructions are given for keeping and studying a wide variety of invertebrates, some fish, amphibia and rodents. The problems covered range from locomotory, feeding and withdrawal responses to simple stimuli (heat, light, vibration, gravity, touch, etc.) to habituation and other forms of learning, orientation in gradients and mazes, problem solving, exploration and the analysis of more complex behaviour sequences, e.g. in courtship.

On the whole, the book gives sound advice and contains numerous useful practical hints; many of the experiments described could well be used to supplement the orthodox approach to zoology via morphology and physiology. However, as it is not always made sufficiently clear what points a suggested experiment tries to solve, or how conclusions drawn can be related to the biology of the species concerned, students would have to be given considerably more information by their instructors. The book can be recommended as a welcome addition to the small number of guides in the field, and its annotated bibliography should be helpful as an introduction to original literature.

U. WEIDMANN

Science and ESP. Edited by J. R. SMYTHIES. London: Routledge & Kegan Paul. 1967. Pp. viii + 306. 42*s*.

There are three positions that a scientist may adopt with respect to paranormal phenomena: (*a*) he may ignore them or deny that they exist (and so long as the phenomena are rare and elusive this is likely to remain the favourite position); (*b*) he may treat them as manifestations of some transcendental principle in nature lying outside the scope of scientific explanation in the strict sense; or (*c*) he may acknowledge them and seek to readjust his conceptual framework to accommodate them. All the contributors to this compendium seem prepared to accept ESP as a fact and concentrate on its implications. Their standpoints vary as between (*b*) and (*c*); perhaps the most explicit statement of (*b*) is given by a philosopher, H. H. Price, in his 'Psychical Research and Human Personality', while (*c*) finds support from the two physicists among the authors. Thus, Henry Margenau in his 'ESP in the Framework of Modern Science' argues that developments within physics have made the older dichotomy between normal and paranormal increasingly gratuitous; Adrian Dobbs goes much further and, in his 'The Feasibility of a Physical Theory of ESP', takes certain concepts current in advanced quantum theory, such as particles of mathematically imaginary mass, and shows how they might be used to construct a theory of telepathy and precognition. He even claims that his theory is sufficiently precise to allow for the prediction of a particular EEG effect, and it is to be hoped that it will eventually be put to the test.

Some of the articles in this collection have been disinterred from the learned journals, including Gilbert Murray's 1952 Presidential Address to the S.P.R. Rereading it, I could not help thinking how remiss it was of this great scholar never to have submitted his own unique telepathic gifts to investigation but to have remained content to use them to play parlour-games inside the family circle. The longest as well as the most learned article in the book is the one by Sir Cyril Burt, who attempts to dispose, one by one, of all the *a priori* objections to ESP. The remaining contributors include a zoologist (Sir Alister Hardy), a psychiatrist (J. R. Smythies), an anthropologist (Francis Huxley) and sundry others. Nothing that they have to tell us, however, is likely to upset the general belief that, so far, parapsychology has had no appreciable effect on the development of science, or even of psychology, but they do demonstrate once again that many thoughtful persons are convinced that ESP is important if only we could make sense of it.

JOHN BELOFF

Histoire de l'hypnose en France. By DOMINIQUE BARRUCAND. Paris: Presses Universitaires de France. 1967. Pp. 236. F. 12.

The history of hypnosis is a fascinating one, and Dr Barrucand's book is a fascinating volume which traces the development of hypnosis in France from Mesmer's early adventures in 'magnetism' up to Janet's death in 1926. Thereafter, as Dr Barrucand remarks, there was little interest in hypnosis in France, with a few exceptions, until the 1950s, when there was a revival of interest.

It is extremely interesting to trace the nature of thought about hypnosis and the phenomena, real and imaginary, with which it was historically associated, through the various stages and controversies—first between the 'fluidists' and the 'animists' and later between the physiologically orientated Salpêtrière school of Charcot and his followers and the psychologically orientated Nancy school of Liébeault and Bernheim—and to follow the development of what was eventually to become psychotherapy. It would, perhaps, have been an improvement if Dr Barrucand had dealt at rather greater length with the main figures in this development. For example, a little space might have been devoted to the vacillation of Liébeault himself on the physiological–psychological issue. There is, however, a special fascination in reading about some of the minor figures in this history, and their bizarre notions. There is, for example, the 'metallotherapy' of Burq about the middle of the nineteenth century, the earlier procedures of a M. Segrétier of Nantes (described by Deleuze in 1825—he 'placed a *baquet* or tub between four magnetized trees joined to each other and to the *baquet* by ropes; he found this of great value, and erected a thatched roof between the trees to shelter patients from the rain'), and the later provocation by Luys of symptoms of nausea, drunkenness, etc., by means of tubes filled with ipecacuanha, alcohol, etc., held against the back of the subject's neck. There is also the notion of the 'transposition' of the senses, and of extra-ocular vision, which was recently revived in Russia.

Apart from the considerable intrinsic interest of this book, it should also serve to remind us that, in the face of human credulity, we need to insist upon the application of the strictest standards of empirical validation to all claims in the field of human psychology and behaviour.

D. GRAHAM

Physiological Psychology. By PHILIP TEITELBAUM. Englewood Cliffs, N.J.: Prentice-Hall. Pp. viii + 120. 18*s*.

At a first impression it might be felt that such a modest volume could hardly do justice to the vast and increasingly complex field of physiological psychology. It could be assumed that Professor Teitelbaum had indulged in gross over-simplification or very narrow selection. In fact, he has simplified and selected, but his simplification does not lead to over-much distortion and his selection is from a very wide field indeed.

The book contains 11 short chapters which could almost be regarded as independent papers. Some, such as the two chapters on 'Feeding' and 'Regulation and Motivation', contain considerable detail, even although the area covered is quite broad, while others, particularly that on the 'Developmental Synthesis of Behavior', are disappointingly general. The basic approach to each chapter is to consider a problem area, usually one where recent work has created interest and insight, and to develop the topic through a historical account up to the state of understanding at the present. This serves to pinpoint the problems particularly clearly where, as in the case of Hartline and Ratliff's work on Mach bands, the work is at an advanced stage of 'simple' sophistication. In other areas, however, such as studies of the limbic system and its relation to emotion, limitation of space has led to a very brief and muddy account. More space might have led to more clarity and a better understanding of the real problems in this area.

One basic lack in the book is a chapter on basic nervous system anatomy. The total novice would be unable to relate structures to each other and to place them in the nervous system as a whole. It is therefore impossible to recommend this book as a sole introductory text. As a book for students with some very basic knowledge of neuroanatomy and physiology it should prove stimulating, at least in parts.

R. S. CORTEEN

'Instinct' and 'Intelligence': The Science of Behaviour in Animals and Man. By S. A. BARNETT. London: MacGibbon & Kee. 1967. Pp. xiv + 250. 45*s.*

This book is aimed primarily at the intelligent layman and attempts to illustrate the present state of research on animal behaviour and its relevance to the study of human behaviour. Recently there have been a number of semi-popular books which have emphasized the animal origins of human behaviour. By ignoring half the evidence, these have managed to portray man as intelligent but wayward, driven hither and thither by tendencies inherited from his territorial ancestors. Such opinions have attracted a great deal of attention and one must immediately welcome Barnett's book as a most necessary antidote, for the view it takes is balanced and realistic.

Barnett's other books have been noted for their clarity of style and this one is no exception. Throughout it reads attractively and keeps up a good flow of unhackneyed examples and crisp comment which compels attention. On the whole it successfully steers a path between the over-technical and the over-simplified and most of my criticisms are relatively minor ones.

Books on behaviour pose particular problems, both of content and of order of presentation. Barnett, quite rightly at this level, has tried to include both some physiology and some ecology. He begins with a short chapter called 'Understanding Animals' which outlines the possibilities for a scientific approach. This leads into a concise account of sense organs and nervous systems in mammals, followed by a comparative survey of these systems through the main animal phyla. Although these chapters are well done, neatly interweaving structure and function, they are quite lengthy and I feel the book loses some of its impetus for the general reader by putting the nervous system before the behaviour. This is particularly felt because, when he does introduce behaviour proper in the next section, Barnett does this excellently. He begins with chapters on 'Movement in the Living Space' and 'Search and Exploration' in which the analysis of behaviour is allowed to grow naturally from quite commonplace observations of how animals move about. The chapters which follow include good accounts of ethological work on reproductive and social behaviour, and development—dealing clearly with the interlocking roles of heredity and environment. The section on learning and intelligence is perhaps a little thin but it makes most of the important points. The concluding chapter is on modern work with primates and its implications for the study of human behaviour.

The book is well illustrated and includes both useful selected references and a good index. I hope it will be widely read, but it is probably too expensive and I feel this kind of book should be published as a cheap paperback if it is to attract the full audience it deserves.

AUBREY MANNING

Signal Detection Theory and Psychophysics. By DAVID M. GREEN and JOHN A. SWETS. New York: Wiley. 1966. Pp. xi + 455. 104*s.*

In the last 10 years or so signal-detection theory has become a very pervasive influence in experimental psychology. First applied in basic psychophysical situations, the model has now been widely extended to studies of perception, memory, personality and skill. This book, by two of the model's original psychological exponents, describes the theory and assesses its current status.

As the model was developed from work in mathematical statistics and communication engineering, the authors devote some space in appendices to an introduction to probability theory, statistical decision theory and waveform analysis. The conscientious reader can thus equip himself for a fairly full understanding of the theory's development while those who merely wish to learn what the theory is about and how it might be applied can take this theoretical background on trust. There is a further useful appendix on experimental techniques.

The main body of the book is organized into three sections, the first of which deals with decision processes in perception. Statistical decision theory is applied to the detection of faint signals and the idea introduced that the observer behaves like a statistician who sets up rules to decide whether the sample of stimulus information arose from a signal distribution or from a distribution of noise. The authors devote a chapter to the nature of these distributions; in particular, they discuss the validity of the assumption that the distributions are normal. Experiments illustrating the basic uses of the model are described and the section ends with a discussion of two major contributions of the theory: the possibility of separating sensitivity from criterion factors and the rejection of the threshold concept, in its simple form at least.

The second section deals with sensory processes in detection. Mathematical models have been constructed to specify the behaviour of an ideal observer in this situation—these models are discussed and the performance of human observers is evaluated against them. The section ends with a description of a psychophysical model for auditory energy-detection. This part of the book is the only one which is heavily mathematical but even here the ideas are so lucidly set out that the non-mathematical reader should have no difficulty in following the argument.

A final section deals with applications of the theory. There is some interesting discussion of the 'critical band' concept—the idea that the ear acts as a narrow filter and that only noise within the critical bandwidth of the filter is effective in masking the signal. The authors also develop the similarity between critical bands and the 'searchlight' notion of attention. Many experiments are described and evaluated in the light of various models which have been suggested. Signal-detection theory has been applied for several years to speech communication and more recently to a wide variety of topics in experimental psychology—reaction times, vigilance, attention and memory, amongst others. As interest in applying the model has risen dramatically in the last two or three years, it is likely that this section will date more quickly than the rest of the book.

It is difficult to praise too highly this review of the theory and its applications. The index is full enough for the book to be used for reference; despite the difficulty of the material, both style and general presentation are extremely clear, making it a very readable textbook; with the appendices and a selection of problems after most chapters, it may also be used for teaching. The reader who is merely interested in finding out in general terms what the theory is about would perhaps be better advised to go to a short account in the journals but even he would profit from the section on applications. For someone working in psychophysics the book is a 'must'.

F. I. M. CRAIK

Men in Middle Life. By KENNETH SODDY (with MARY C. KIDSON). London: Tavistock. 1967. Pp. xii+485. 63*s*.

Perhaps the most concise judgement that one could make about this book is that it is written 'around' the subject of men in middle life. This is not to say that the authors avoid the central issues, but rather that they are so concerned to describe the context that the central issues are often badly obscured. The aim is to provide an ideal multidisciplinary and cross-cultural approach to the study of the mid-life period. Human ageing, however, is a notoriously difficult research area, and most investigators are forced to compromise when it comes to dealing with awkward methodological and administrative obstacles to research. *Men in Middle Life* is a detailed statement of an ambitious research policy which might eventually have overcome some of these obstacles and might have led to important developments in knowledge if the original scheme of the World Federation of Mental Health to set up research and study centres had not failed for want of financial support.

The book raises a great many general issues and discusses at considerable length the biological, social and psychological background of these issues. It will be for the individual reader to decide which of the many side-issues are relevant, or which of them are dealt with at too great a length; there is also much repetition, the omission of which would have been beneficial. Few, if any, of the central issues represent original insights into human ageing generally or the mid-life period in particular, but some of the contextual material may provide new ways of looking at old problems.

The contents of the book include a general introduction to the problem, chapters on facts and fantasies about ageing, normal and deviant development, growth rates and sequences, family interaction between generations, social change and social responsibility. The last two chapters constitute a kind of review of material presented earlier. The appendix contains a report on 'Tradition Learning in Animals', the title of which has been omitted from the list of contents.

The appendix also contains two bibliographies—one of works in English and one of works in other languages. It is always useful to have bibliographies, but it would be a mistake to suppose that the selection of titles was much more than a rough sampling of a very extensive literature. A number of minor errors seem to have passed unnoticed in the final editorial checking.

D. B. BROMLEY

Graduate Education in Ageing within the Social Sciences. Edited by ROSE E. KUSHNER and MARION E. BUNCH. Ann Arbor: University of Michigan Press. 1967. Pp. x+118. $5.00.

In Great Britain, postgraduate courses as a whole are still in their infancy, and none as yet exists in gerontology. This book gives an overview by 16 American gerontologists—mainly psychologists but also some sociologists and economists—of existing training in social gerontology for American graduate students, and some discussion of their hopes for the future of such training.

The need for people to carry out research and service work in ageing is proportionately as great in this country as it is in the United States. The need to be able to offer long-term career prospects to young gerontologists is already felt here as strongly as there. Thus the two countries do have common problems to solve.

But they do not have a common academic culture; their philosophy of undergraduate education is very different. The British graduate has, on the whole, a good grounding in all areas of his honours subject, though he has usually done little in other subjects. The American system produces graduates who, on the whole, require further grounding in the basic material of their 'major', though they will also have covered a wide range of other subjects at a non-specialist level. The postgraduate requirements in the two countries are thus likely to be very different, and the American experience, while interesting, cannot be applied directly to this country.

Gerontology is an area which cries out for multidisciplinary research, not only within the social sciences, but also cutting across the boundaries of the social, biological and even physical sciences. The Americans favoured co-operative research between people trained as postgraduates in a single discipline—time precluding multidisciplinary training in depth for them. If British postgraduate courses in gerontology were to be set up, the issue would have to be re-argued in the British context, and some multidisciplinary training might, one hopes, prove more viable here than there. This book brings plenty of issues of this nature out into the open, and therefore might on occasion be very useful.

SHEILA CHOWN

Psychological Time in Health and Disease. By JOHN COHEN. Springfield, Ill.: C. C. Thomas. 1967. Pp. xv+103. $6.50.

Like time itself, this book is somewhat paradoxical. It is published as a monograph in a series of American Lectures in Living Chemistry, said to be conceived 'to advance the newer knowledge of chemical medicine in the cause of clinical practice'. Yet it contains no discussion of chemistry at all, whether that involved in circadian rhythms and 'biological clocks', or that of the neurochemistry of drugs affecting time judgement; nor is there any evidence that its contents were ever presented as lectures. With the exception of one chapter, it singularly lacks the standards of scholarship one has the right to expect from a monograph. The level of discussion ranges from the journalistic to the academic, from the experimental to the speculative, often stimulating but sometimes irritatingly superficial, as Professor Cohen endeavours to cover far too much ground in less than 90 pages of text. The book has a somewhat catchpenny title, directed no doubt to psychiatrists and to medical librarians, which the contents fail to justify. As one expects from this publisher, the physical quality of the book is excellent (as it ought to be for the price), but the typography is undistinguished, the proof-reading poor, and this slim volume is padded out by first printing references in full at the end of each chapter, and then collecting them together as a bibliography at the end of the book.

Cohen begins with a chapter entitled 'An Internal Clock', in which he reviews briefly the hackneyed work of Piéron, François, and Hoagland on the relationship between body temperature and the rate of subjective time, and takes a glance at circadian rhythms, 'biological clocks', accuracy of time judgement under hypnosis, and on awakening from sleep. Chapter 2, on the micro-structure of psychological time, covers the contributions of Mach and Meumann, the indifference interval, information theory and the irreducible moment, variations in judgement technique depending on interval length, the scaling of apparent duration, and an interesting note on Cohen's own work on the use of psychological time as a measure of subjective probability. Chapter 3 provides a cursory survey of judgements of the past and future, but ignores almost all the considerable experimental work on temporal perspective, the influence of social class, intelligence, and so on. Though brightly written, these chapters are inadequate, with a selection of evidence suggesting that the author simply does not know the literature thoroughly, leading to oversimplifications which even the extreme brevity cannot justify. Perhaps more importantly for the non-specialist to whom this book is presumably directed, many basic problems are ignored. For example, there is no discussion of the logic or psychophysical status of the various methods of time estimation, nor of whether they are all measuring the same thing. No hint is given of the importance of accurate timing in skilled behaviour and in much other psychophysiological regulation, or of the role of many 'biological clocks' in simply contributing to this regulation. There is no discussion of the controversy over endogenous *vs.* exogenous factors in biological chronometry; no conceptual clarification of a 'clock', or warning of the many different types of system loosely classified as 'physiological clocks'; no comment on whether some master chronometer underlies personal tempo, or how this might be related to time judgement; and no suggestions for experiments to test a 'biological clock' hypothesis in relation to psychochronometry. No information is given on studies of the role of heredity in personal tempo and in time estimation (initiated by Frischeisen-Köhler in 1933), probably the most rewarding line of investigation at the moment for those seeking the structural basis of timing; nor is one told of the attempts of Wiener, Goody, Werboff, and others to locate the 'hands' of the internal clock in the alpha-rhythm.

However, in chapter 4, on the relativity of psychological time, Cohen provides an excellent brief review of the tau and kappa effects; the relationship of estimates of duration, distance and speed; and of the controversy between Piaget and Fraisse about whether children have a primary intuition of time. For the undergraduate this summary could hardly be surpassed, but for others it is disappointing that Cohen has nothing new to say in an area which has gained greatly from his own experimental work.

The following chapter, on aberrations of psychological time, is presumably the justification for the inclusion of 'disease' in the title of the book. By ordinary academic standards it is unsatisfactory. A great deal is now known about the psychopathology of time, the influence of drugs on time judgement, and the effect of brain damage and other organic pathological states on psychological time. It is true that these observations are scattered in a wide variety of journals, are often incidental to the main purposes of the studies, and have not yet been adequately structured in a monograph. But the psychologist interested in time should be aware of them, and should be well able to present a review of subjective time in disease states which would be of clinical and experimental value. Chapter 6 gives an entertaining but largely derivative account of subjective time in myth and literature, important certainly, but only peripheral in this context. Finally, chapter 7 makes some useful comments on Treisman's model of an internal clock.

The failure of this book stems from attempting too much in a brief compass; the author dazzles by breadth of reading rather than by depth of scholarship. To consider it as a monograph is misplaced, for the range of topics touched upon requires a whole series of specialist reviews. These are urgently needed for the synthesis of the vast mass of scattered data on psychological time, and for its integration into the main stream of psychological investigation. Professor Cohen would win more acclaim from those of us who are equally 'time-haunted' (in J. B. Priestley's phrase) if he would abandon attempts to reach the general reader, and buckle down to giving us a scholarly monograph devoted solely to the relativity of psychological time. If others would do likewise in related areas it might *then* be worth while to consider attempting a comprehensive general text, the ideal author for which might well be found in Manchester.

EDWARD GRANT

The History of Psychiatry. By F. G. ALEXANDER and S. T. SELESNICK. London: Allen & Unwin. Pp. xvi + 471. 63*s*.

The story of psychiatry is a bizarre and fascinating one. As it concerns man, his attempts to understand his own behaviour, and that of the ever-present band of disturbing non-conformists—the mentally ill—it should be the most interesting story of all the medical disciplines. Fortunately, the authors of this book avoid the commonest pitfall of medical histories: the tedious chronicle of names, dates and discoveries. They have written an entertaining and informative book by putting the developments in psychiatry into a wider context of great historical events. Man's changing attitude to mental deviation and his methods of coping with it are seen against a background of religious, cultural and political movements taking place at the time. This imaginative and scholarly perspective gives greater meaning to the slow, painful progressions, and the frequent, brutal retrogressions of psychiatric thought. Yet somehow, as history, this book seems to lack coherence. Perhaps it is too early in the evolution of psychiatry, which, after all, has not long enjoyed legitimacy within the family of medical specialities, or for that matter a rational, scientific basis, for there to be a reasonably stable vantage point from which to perceive a pattern, a course of events, which is the essence of historical analysis.

Although the authors announce their first chapter with the title 'Psychiatry Comes of Age' and write of 'the advantage of a present-day perspective', this assumption—that psychiatry has somehow 'arrived'—is debatable, especially when one considers the paucity of rational aetiological theories and treatments available in this field. Consequently, it seems to me that, considered strictly as history, the selection of material here is occasionally contrived and arbitrary. But as a story about people concerned in differing ways with 'mind' and 'body' and their relationship—witchdoctors, philosophers, theologians, psychologists, doctors, and exotic characters of genius including saints and villains, scientists and charlatans—the book makes absorbing reading.

As it is, one-third of the text is devoted to a useful, if unavoidably selective, review of recent and current developments in psychiatry. The history itself begins with man's written records of four or five thousand years ago, and the theories and remedies of the ancient, classical, medieval, renaissance and romantic periods are vividly described. These pages are packed with interesting information, anecdotes and thumb-nail sketches about men as varied as Imhotep, the Egyptian healer who lived about 2850 B.C., Hippocrates, Asclepiades, St Augustine, Heinrich Kraemer, Robert Burton, Charcot, Pinel and Virchow—to mention but a few. Not unnaturally, given the orientation of the authors, the chapters on 'The Freudian Era' constitute the pivot of the book, and it is a very readable section. It is always possible, given people's biases and predilections, to quibble over the space devoted to particular men and their ideas. However, it does seem disproportionate to devote only about three (rather dismissive) pages to Kraepelin—in the section 'The Modern Era'—in comparison with the 37-odd pages justifiably given to Freud and his movement.

It is a pity that this distinguished book should end with an appendix entitled 'Jung and the National Socialists', and which contains, among other deprecatory statements, the following: '...Jung made concessions to the Nazi racial philosophy, which supports the impression that he lacked Freud's uncompromising moral fortitude and also Freud's scientific objectivity'. Is this a valid footnote for a history of psychiatry, or the unnecessary echo of those personalized feuds which make it so difficult to believe that psychiatry has as yet come of age?

M. HERBERT

A History of Psychology in Autobiography, vol. V. Edited by E. G. BORING and GARDNER LINDZEY. New York: Appleton-Century-Crofts. 1967. Pp. xii + 449. $8.00.

This volume follows the pattern of vols. I–IV. Each contributor tells the story of his life, his university education and the steps which led him to become a psychologist and to tackle the problems on which his reputation rests. The influences from family background and early mentors are clearly defined, but what is lacking are the subtler socio-cultural influences which impinge on scientific thinking, important in a subject like psychology which might be expected to be peculiarly sensitive to historical movements of a more general kind.

The off-the-cuff reflexions are interesting. J. J. Gibson is pessimistic: psychologists, he says, 'seem to feel...that all we need to do is to consolidate our scientific gains. Their self-confidence astonishes me. For these gains seem to me puny, and scientific psychology seems to me ill-founded. At any time the whole psychological applecart might be upset. Let them beware!' B. F. Skinner, who reveals an attractive wit, is confident. Behaviourism, he says, 'may need to be clarified, but it does not need to be argued. I have no doubt about the eventual triumph of the position...'

With Allport, Murray, Guilford, Rogers, Goldstein and Gardner Murphy included, the study of personality is given its place in all its diversity of theory and methodology. One is reminded on reading through this collection of O. L. Zangwill's aphorism in justifying his collection of material in a popular article in 1956: 'Every psychologist has perforce to create his own psychology'.

The contributors are: G. W. Allport, L. Carmichael, K. M. Dollenbach, J. F. Dashiell, J. J. Gibson, Kurt Goldstein, J. P. Guilford, H. Helson, W. R. Miles, Gardner Murphy, H. A. Murray, S. L. Pressey, Carl Rogers, B. F. Skinner, M. S. Viteles.

R. THOMSON

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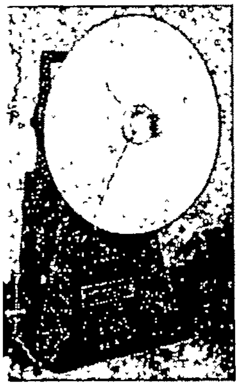

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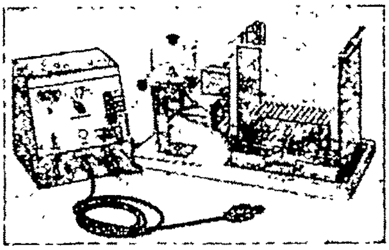

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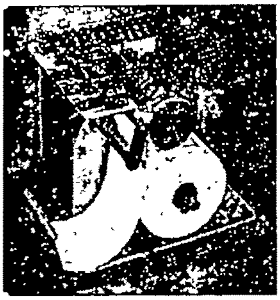

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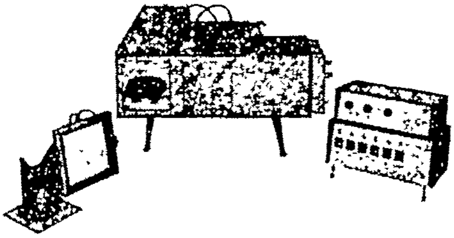

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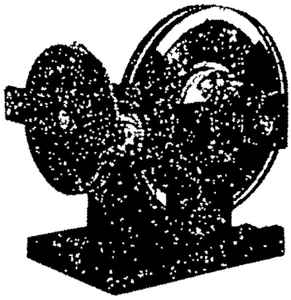

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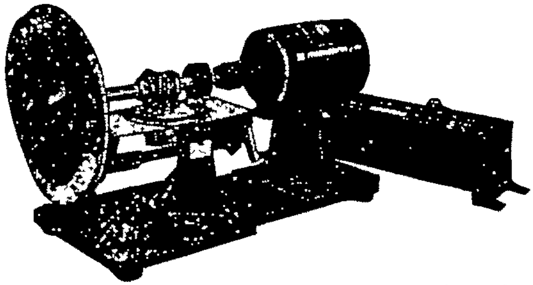

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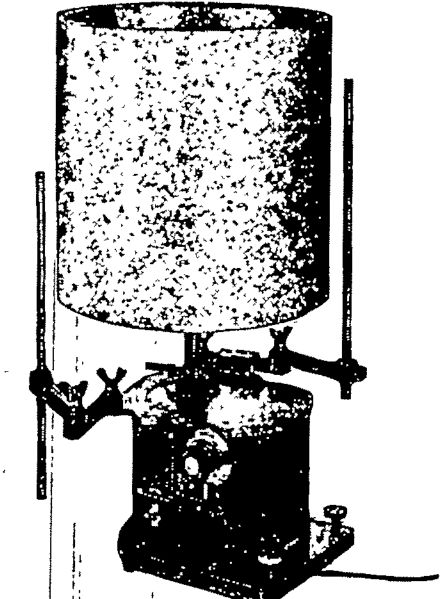

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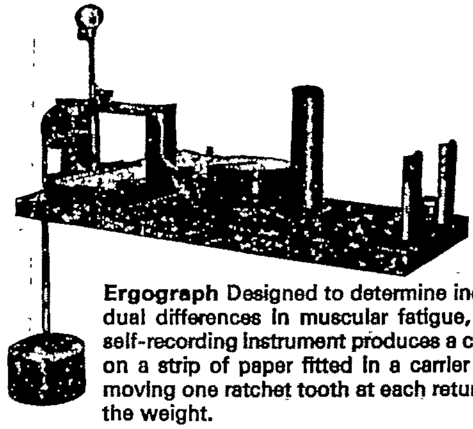

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PROPOSED EUROPEAN BRAIN AND BEHAVIOUR SOCIETY

We, the undersigned, wish to propose the formation of a EUROPEAN BRAIN AND BEHAVIOUR SOCIETY. The objects of the Society shall be the furtherance of scientific enquiry within those fields that bear on the interrelationships between brain and behaviour by holding periodic meetings at which papers are read and discussions held, by the dissemination of information and educational materials made available as a consequence of research in the interrelationship of brain and behaviour, and by such other activities as may be decided upon by the Society.

A study group will meet in Rotterdam in the Spring of 1969 in order to continue our discussions in detail of the aims of the proposed Society and the direction in which it shall move, to consider and adopt its constitution, and to select its first membership. All persons interested in becoming a member of the Society are invited to write as soon as possible to Dr A. Cowey (Institute of Experimental Psychology, South Parks Road, Oxford, England) stating their qualifications, experience, present work and interests. In addition, it would be appreciated if any opinions and suggestions about the general aims of the proposed Society could be expressed at the same time, and if applicants would state whether they are willing to deliver a paper at the first meeting of the Society, reporting scientific work that is considered to be in line with the aims of the proposed Society.

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IS THERE A MOTOR-CONSTANCY MECHANISM?

BY D. LEGGE AND JUDITH M. POTTINGER
Department of Psychology, University College, University of London

Two experiments were conducted to investigate the extent to which subjects can compensate for the effects of elbow angle on the mechanical efficiency of the elbow muscle-joint system. Subjects were found to be able to match movements made in different directions and made from different starting-points but unable to match isometrically produced torques made under comparable conditions. The results are compatible with the hypothesis that subjects possess a limited motor-constancy mechanism operating on the basis of temporal modulation of muscle activity.

Organisms depend for survival on their ability to respond to and modify their environment. Most responses change an organism's relation with its environment either by altering its position in the environment by locomotion or by manipulating parts of the environment. The primates, and in particular man, have extremely well-developed manipulatory abilities and their behavioural superiority is largely dependent upon these skills. However, analysis of the action of the peripheral response mechanism suggests that it is ill-designed to produce effective environmental changes.

Man manipulates his environment mainly with his upper limbs. These are systems of articulated linkages controlled by antagonistic sets of muscles. Movement of part of the system with respect to an adjacent part occurs when the combination of muscular forces acting about the joint in question exerts a non-zero torque. The elbow joint may be used as an example. This joint closely approximates a simple hinge, and movement of the forearm about this joint is controlled by flexor and extensor muscles acting antagonistically. Since the behaviourally valuable response is an environmental change it would be efficient if a constant efferent signal to these muscles produced a constant effect on the environment. The physiological properties of muscle and the physical properties of joint geometry preclude such a simple relation. The tension developed by a muscle receiving a constant efferent signal varies with its length (Hill, 1938); the shorter the muscle, the less the tension developed. As the elbow bends, not only do the flexor muscles shorten, and so develop less power for a constant input, but, in addition, the mechanical advantage of the muscle–joint system changes, reaching a maximum, for example, when the flexors act perpendicularly to the forearm. These two factors—one physiological, one physical—imply that to produce a constant force on the environment (or, equivalently, a constant torque about the elbow joint), continuous compensatory modulation of the efferent signal to the muscle would be required.

A mechanism which introduced automatically such compensatory modulation would have obvious biological utility. Such a motor-constancy mechanism would preserve the relation between intent and effect in a way analogous to the visual constancies which maintain constant the perception of size and shape of perceived objects, though their distance from and orientation to the observer may change.

As the visual constancies promote accurate interpretation of the environment, motor constancy would facilitate precise manipulation of the environment.

In order to function, a motor-constancy mechanism would require information about the state of the effectors. This information is available from proprioception, which includes both a muscle sense and a joint sense. The muscle sense is served by the Golgi tendon receptors which signal the tension developed by the muscle acting on the tendon. The joint sense depends upon receptors in the joints, which signal the angle of articulation of the joint. There are two kinds of joint receptor: tonic receptors giving a steady signal for a fixed joint angle, and phasic receptors which adapt rapidly and thus are most active while the joint angle is changing (Boyd & Roberts, 1953; Skoglund, 1956; Howard & Templeton, 1966). The joint sense would be theoretically sufficient to sustain a motor-constancy mechanism because specification of the joint angle implies both the length of the muscle and the mechanical advantage of the muscle–joint system. However, the muscle sense would provide a valuable monitoring system which would help to stabilize performance.

To be perfectly efficient, motor constancy would have a perceptual as well as a motor aspect. It would be desirable that while a constant effect on the environment was being produced by appropriate modulations of the efferent signal, such a constant effect was also perceived to occur. This perceptual constancy would demand the integration of muscle-sense and joint-sense information so that produced torque, and hence the effect on the environment, could be inferred from muscle-tension information, which is the only directly available information about muscular performance. Although it is conceivable that one aspect of this mechanism should exist without the other, it seems probable that, if one exists, both do. The motor aspect without its perceptual counterpart would mean that appropriate responses would not feel right, while the perceptual aspect alone would mean that appropriate responses would either have to be learned for an infinity of different situations or be carried out in slow motion.

An operational distinction may be made between the perceptual and motor aspects of this hypothetical constancy mechanism by studying responses which are made too rapidly to be controlled by current feedback (i.e. ballistic responses—Craik, 1947; Begbie, 1959) as well as slower responses which are deliberately controlled on the basis of current feedback. In both experiments the control of the elbow joint was studied. Subjects were to produce a target response and then to match it with a second response. By altering the angle of the elbow joint obtaining when the target and matching responses were made and by changing the primary muscle groups controlling the response (i.e. flexors or extensors), it was possible to determine the extent to which changes in the state of the effector system can be compensated for by appropriate efferent modulation.

EXPERIMENT I

This experiment was designed to investigate the accuracy with which subjects can reproduce isometric muscle contractions in the same and in antagonistic muscle groups and after changes in the elbow angle.

Method

Subjects were required to produce a tension of their own choice in a specified direction while their elbow was held at a pre-set angle. They were then required to match this tension with a second response. The initial tension was produced with the elbow at an angle of 75°, 105° or 135° extension; 180° represented full extension of the elbow. The matching tension was produced after changing the initial elbow angle by $-20°$, 0° or $+20°$. Each tension could be produced by either the flexor or extensor muscle groups. There were thus 36 experimental conditions, all of which were presented in random order to each subject. Two groups of subjects were run: one group produced their responses ballistically, the other non-ballistically. A response completed in less than $\frac{1}{2}$ sec. was classified as a ballistic response (Begbie, 1959; Vince, 1948).

Subjects. Twelve right-handed student subjects were evenly divided between the two groups, three men and three women to each group.

Apparatus. The main part of the apparatus was a 1 in. diameter cylindrical metal bar of silver steel, pivoted at one end, and mounted horizontally on a 3 ft. high vertical steel tube, 8 in. in diameter. The bar could be rigidly locked at any angle. Mounted on opposite sides of the bar were two silicon strain-gauges, which formed part of a Wheatstone bridge circuit. The temperature sensitivity of the strain gauges was compensated for by inserting two thermistors into the circuit. The position of the strain-gauges ensured that any force applied to the bar which tended to bend the bar in the horizontal plane caused a change in the balance of the bridge circuit. Imbalance of this circuit was displayed on a 'Telequipment' oscilloscope after amplification by a differential amplifier. A 10 msec. time base was added to the oscilloscope display from a modified 'Labgear' decatron timer. This display was photographed on continuously moving film with an oscilloscope camera. The time interval between the initial and matching responses was controlled with a 'Nagard' process timer.

Procedure. The subject was seated in a modified dental chair, which was adjusted so that his right upper-arm was horizontal in a plane between the coronal and median planes. His elbow was positioned immediately above the pivot of the steel bar and his forearm supported above and parallel to the bar. The forearm was held in a semi-supine position by a wrist-grip designed to clamp on the wrist bones without interference with nerves and blood vessels. The subject's shoulders were strapped to a back-rest to minimize the contribution to his performance of muscles other than the elbow flexors and extensors.

Subjects were instructed to produce pairs of forces, exerted against the wrist-grip. The first force of each pair was to act as a target to which the second force should be matched. The effects of different forms of instructions were studied in a pilot experiment. It was found that subjects performed in the same way, no matter whether they were asked to match forces or given explicit examples emphasizing the matching of environmental effects. Several practice trials were provided, but knowledge of results was withheld throughout the whole experiment. Subjects required to produce ballistic responses were told to make their responses as quickly as possible and given practice until they were consistently completing responses in less than $\frac{1}{2}$ sec. Subjects producing non-ballistic responses were encouraged to correct their responses until they were satisfied with the accuracy of their matching. The matching response was made 7 sec. after completion of the initial response. After the initial response had been made the elbow angle was always changed passively and then set to the angle appropriate for the matching response.

Results and discussion

The force produced by subjects was converted into lb. in. units of torque about the elbow joint. Matching error was measured as the difference between initial and matching torques, with the convention that a positive difference indicated that the matching response exceeded the initial response. The size of the matching error was found to be an increasing function of the size of the initial response, so, before statistical analysis, the raw matching errors were converted into proportional errors relative to the size of the initial response. Mean proportional error scores, averaged over subjects, are shown for each of the 72 experimental conditions in Table 1.

These proportional error scores were submitted to an analysis of variance, which is summarized in Table 2.

There are four main conclusions to be drawn from this experiment. First, the speed with which subjects made their responses (i.e. ballistically or non-ballistically) had no effect on performance. Furthermore, there were no interactions between speed of response and any of the other independent variables.

Table 1. *Mean errors in torque matching expressed as a proportion of the initial produced torque, averaged over subjects, for each of the* 72 *experimental conditions*

(Ballistic and non-ballistic responses were made by separate groups of six subjects.)

		Muscle groups			
Initial response ...		Flexor		Extensor	
Matching response ...		Flexor	Extensor	Flexor	Extensor
Initial elbow angle	Change in elbow angle				
Ballistic					
75°	−20°	−0·04	−0·37	+0·35	−0·05
	0°	−0·02	−0·33	+0·37	+0·04
	+20°	−0·18	−0·30	+0·36	−0·14
105°	−20°	+0·25	−0·19	+0·37	−0·06
	0°	+0·07	−0·07	+0·23	−0·04
	+20°	−0·20	−0·40	−0·03	−0·17
135°	−20°	+0·21	−0·18	+0·27	+0·03
	0°	−0·01	−0·26	+0·36	+0·04
	+20°	−0·15	−0·22	+0·18	+0·04
Non-ballistic					
75°	−20°	+0·04	−0·36	+0·28	−0·02
	0°	+0·05	−0·38	+0·44	−0·02
	+20°	−0·19	−0·42	+0·39	−0·13
105°	−20°	+0·12	−0·28	+0·65	+0·03
	0°	−0·04	−0·31	+0·01	−0·03
	+20°	−0·08	−0·35	+0·04	−0·09
135°	−20°	+0·17	−0·13	+0·48	+0·04
	0°	−0·01	−0·14	+0·33	+0·01
	+20°	−0·10	−0·14	−0·03	+0·07
	Means	−0·006	−0·268	+0·281	−0·025

Secondly, the muscle groups used for making the initial and matching responses had profound effects. These could be traced to the fact that if the same muscle group was involved in both responses, the matching error was negligible (0·6 per cent for flexor responses and 2·5 per cent for extensor responses), but if flexors were used for the initial response and extensors for the matching response, the average matching error was −26·8 per cent, while if a flexor response was matched to an initial extensor response, an average error of 28·1 per cent occurred. These results show that subjects did not match torques produced by different muscle groups. However, they may have been matching muscle tensions, and the torque differences could have been caused by the different mechanical advantages of the flexor and extensor systems.

Thirdly, the effect of the muscle group used for making the matching response interacted with the change in elbow angle between initial and matching responses.

This relation is illustrated in Fig. 1, which shows the effect of angular change on each of the four combinations of muscle groups used to produce the pairs of responses. It can be seen that if the extensor muscles are used for the matching response, changing the angle of the elbow has a relatively small effect. However, if the matching

Table 2. *Summary of analysis of variance of proportional torque-matching error scores*

Source	D.F.	S.S.	M.S.	F	Significance
Between subjects					
Speed of response (S)	1	0	0	< 1	n.s.
Error between subjects	10	1·55			
Within subjects					
Muscle group (MG)	3	16·12	5·37	69·12	$P < 0{\cdot}001$
Initial muscle group (I)	1	7·45	7·45	91·67	$P < 0{\cdot}001$
Matching muscle group (M)	1	8·64	8·64	106·28	$P < 0{\cdot}001$
I × M	1	0·03	0·03	< 1	n.s.
Initial elbow angle (A)	2	0·32	0·16	1·98	n.s.
Angular change (C)	2	1·88	0·94	11·55	$P < 0{\cdot}001$
MG × A	6	1·09	0·18	2·24	$0{\cdot}05 > P > 0{\cdot}025$
I × lin. A	1	0·35	0·35	4·37	$0{\cdot}05 > P > 0{\cdot}025$
MG × C	6	0·73	0·12	1·50	n.s.
M × lin. C	1	0·71	0·71	8·80	$0{\cdot}01 > P > 0{\cdot}001$
A × C	4	0·67	0·17	2·06	n.s.
MG × A × C	12	1·08	0·09	1·11	n.s.
Pooled interactions with S	35	1·19	0·03	< 1	n.s.
Residual	350	28·44	0·08	—	—
Total	431	51·54	—	—	—

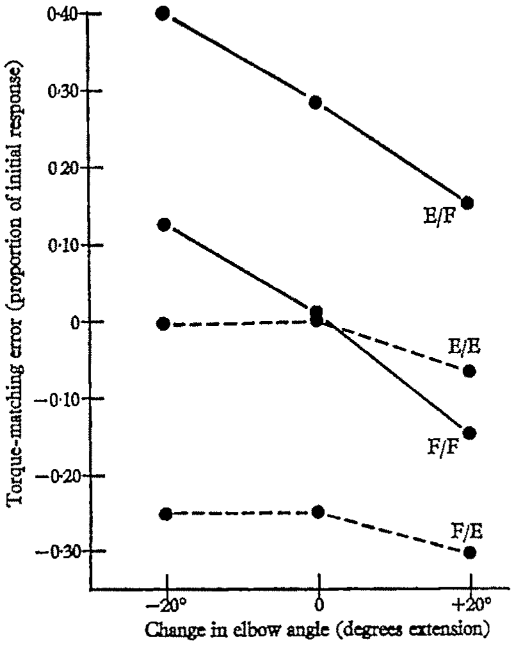

Fig. 1. Mean proportional torque-matching error as a function of change in elbow angle between initial and matching responses. Pairs of responses were produced by extensor–extensor (E/E), extensor–flexor (E/F), flexor–extensor (F/E) or flexor–flexor (F/F) combinations of muscle groups.

response is produced by the flexor muscles, changing the elbow angle has a large effect. This interaction may be interpreted as a reflexion of the different ways in in which mechanical advantage varies for the extensors and flexors. The mechanical advantage of the extensor system changes relatively little with elbow angle, since the lower tendon of the extensors passes around the olecranon process of the elbow which acts as a pulley. In consequence, a constant extensor tension produces a near-constant torque even though elbow angle changes, but a constant flexor tension produces a torque which varies considerably as elbow angle, and hence mechanical advantage, change.

Table 3. *Mean proportional torque-matching errors for matches to initial responses produced by flexor and extensor muscle groups with the elbow set at different initial angles*

Initial elbow angle	Muscle group	
	Flexor	Extensor
75°	−0·208	0·156
105°	−0·124	0·076
135°	−0·080	0·152

The fourth effect exposed by the analysis of variance was an interaction between the muscle groups used for the initial response and the angle of the elbow obtaining when it was made. This interaction is shown in Table 3. The pattern of this interaction is in many respects similar to that between the muscle groups used to produce the matching response and the change of elbow angle between initial and matching responses. However, this interaction relates to the initial response and the initial elbow angle. It appears that the proportional error is influenced by the initial response. A possible explanation of this interaction depends upon the assumption that subjects match their second response to an intended or implicit response rather than to the initial response itself. If this is what happens, the proportional error will reflect the difference between the intended and actual initial response. Unless a correction intervenes, the actual response will vary with the mechanical advantage of the effector system even though a constant response was intended. Since the mechanical advantage of the flexor system changes more with changing elbow angle than does that of the extensor system, the observed interaction of matching errors would be manifested.

In short, the experiment has shown that the physiological and physical properties of the elbow muscle–joint system produce lawful effects on the accuracy with which subjects match isometric responses. The question arises as to whether these influences should affect the accuracy with which subjects can match movements. A ballistic movement—that is, a movement which is pre-programmed and executed without modification based on current feedback—is produced by the production of a temporally ordered sequence of modulations of muscle tension. Since a major factor in the action of the effectors is the mechanical advantage of the muscle–joint system, and since this mechanical advantage may alter considerably as the elbow angle changes, it seems probable that the patterns of error described above should also appear when subjects attempt to match movements. This prediction depends on the assumption that movement of the forearm is simply an integration of elbow torque over time. Since subjects seem incapable of matching torque, they should have equal difficulty in matching movements. Of course, this effect would not be anticipated if subjects responded non-ballistically and modified their responses in terms of perceived movement.

EXPERIMENT II

This experiment was designed to investigate the accuracy with which subjects can match ballistically produced movements made in different directions (i.e. flexion or extension) and starting from different degrees of elbow flexion.

Method

The general nature of the experimental task was the same as that in Expt. I. Subjects produced two ballistic movements of the forearm, attempting to match the second to the first. The initial movement was made with the elbow at a standard angle. The matching movement was made after the elbow angle had been altered by −20°, 0° or +20° extension. Each of the two movements was either a flexion or an extension. On each trial subjects were instructed to produce either small movements (*c.* 15° excursion) or 'large' movements (*c.* 30° excursion). There were therefore 24 experimental conditions. Each subject performed twice under each condition, completing 48 trials in random order.

Subjects. There were three male and three female undergraduate subjects, none of whom had taken part in Expt. I.

Apparatus. A horizontal metal arm, pivoted at one end, was mounted on the 3 ft. high, 8 in. diameter, vertical steel tube used in Expt. I. Rotation of the arm drove a potentiometer spindle, and the potentiometer output was fed to the vertical plates of an oscilloscope. A linear accelerometer was attached to the arm, and the accelerometer output was fed through an amplifier to the second beam of the oscilloscope. A continuous record of the movement of the arm and its acceleration was obtained using an oscilloscope camera. A 10 msec. time base was added to the filmed record. The 5 sec. interval between the initial and target responses was controlled with a 'Nagard' process timer.

Procedure. The apparatus was adjusted for each subject as in Expt. I. His elbow was located over the fulcrum of the horizontally rotating arm and his wrist clamped to the arm. The elbow angle from which all initial movements were made was an angle mid-way between full flexion and full extension. Subjects were instructed to match the second movement to the first and were given sufficient practice until they reliably produced appropriately sized large and small responses, and completed their movements in less than $\frac{1}{2}$ sec. Practice at matching movements was also given, but knowledge of results was withheld throughout the experiment.

Results

As in Expt. I, matching error was expressed as a proportion of the size of the initial movement. A positive error indicates that the matching movement was larger than the initial movement. Mean proportional error scores, averaged over subjects, are shown for each of the 24 experimental conditions in Table 4. These data were submitted to an analysis of variance, which is summarized in Table 5.

Two main conclusions may be drawn from this experiment. First, proportional error was affected by the required size of movement. Large movements were underestimated on average by about 2 per cent; small movements were overestimated by an average of 13 per cent. The most plausible explanation of this effect is that it is an artifact of the proportionalization process. If absolute error and the size of the initial movement are related by a linear increasing function passing through the origin, expressing errors as a proportion of the size of the initial movement yields error scores which are independent of the initial movement. However, such independence is not achieved if a different function intervenes. For example, the observed pattern of proportional errors would be produced if absolute errors were a negatively accelerated rather than a linear increasing function of the size of the initial movement.

Table 4. *Mean errors in movement matching expressed as a proportion of the initial produced movement, averaged over six subjects, for each of the* 24 *experimental conditions*

Initial movement...		Flexion		Extension	
Matching movement...		Flexion	Extension	Flexion	Extension
Movements required	Angular change				
Large (c. 30°)	−20°	−0·10	−0·03	−0·22	−0·03
	0°	−0·02	−0·11	−0·09	−0·04
	+20°	0·21	0·07	0·08	0
Small (c. 15°)	−20°	−0·11	0·23	0·01	0·25
	0°	0·28	−0·09	0·10	0·04
	+20°	0·19	0·32	0·29	0·13
	Means	0·075	0·065	0·027	0·058

Table 5. *Summary of analysis of variance of proportional movement-matching error scores*

Source	D.F.	S.S.	M.S.	F	Significance
Between subjects	5	0·38			
Within subjects					
Size of movement (S)	1	0·93	0·93	22·93	$P < 0·001$
Angular change (C)	2	0·76	0·38	9·38	$P < 0·001$
Initial movement (I)	1	0·03	0·03	< 1	n.s.
Matching movement (M)	1	0	0	< 1	n.s.
I×M	1	0·01	0·01	< 1	n.s.
S×C	2	0·02	0·01	< 1	n.s.
S×I S×M S×I×M	3	0·06	0·02	< 1	n.s.
C×I	2	0·04	0·02	< 1	n.s.
C×M	2	0·69	0·34	8·48	$P < 0·01$
Lin. C×M	1	0·45	0·45	11·25	$P < 0·01$
Quad. C×M	1	0·24	0·24	6·00	$P < 0·025$
C×I×M	2	0·22	0·11	2·75	n.s.
S×C×I S×C×M S×C×I×M	6	0·34	0·06	1·50	n.s.
Residual	115	4·67	0·04		
Total	143	8·15			

Fortunately, the other variables in the experiment did not interact with this size variable and therefore the effect of these other variables may be discerned, even though a proportionalization artifact may exist.

Secondly, the effects produced by the combinations of muscle groups used to make the pairs of responses and the change in the angle of the elbow between the initial and the matching responses were not the same as in Expt. I. The predictions about movement errors based on the results of Expt. I were not confirmed. The muscle groups producing the movements had no effect on proportional error. Thus, whereas subjects were unable to match torques across muscle groups, they were able to match movements that, logically, depend upon producing similar torques. In addition, though there was a significant interaction between the change in elbow angle and the muscle groups producing the matching response, the pattern of effects is quite different from that found in Expt. I and is not readily attributable to variations in mechanical advantage. This pattern is shown in Fig. 2. It would appear that the effects of joint geometry have been compensated for, but some other factor has intervened to impair the accuracy with which movements can be matched.

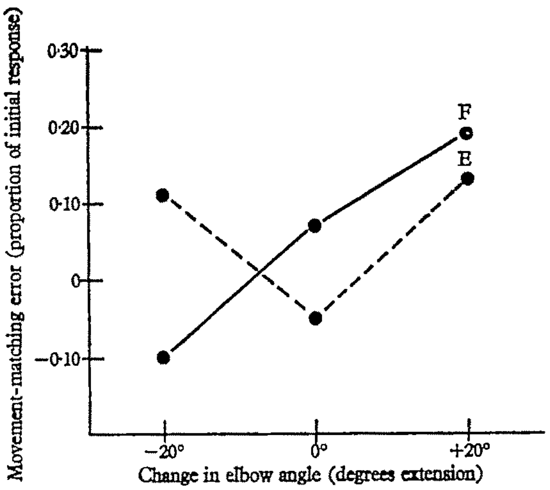

Fig. 2. Mean proportional movement-matching error as a function of change in elbow angle between initial and matching responses, for matches produced by either flexor (F) or extensor (E) muscle groups.

Table 6. *Maximum and minimum levels of torque achieved by subjects required to match isometrically produced forces and to match ballistically produced movements*

	Level of torque achieved (lb. in.)	
	Minimum	Maximum
Isometric muscle contractions	0·88	23·50
Ballistic movements	0·12	4·61

In order to provide a basis for comparing the results of the two experiments, the torque generated during the production of ballistic movements was calculated from the response of the accelerometer attached to the rotating metal arm. The maximum and minimum levels of torque generated during the production of ballistic movements and during the production of isometric muscle tensions are shown in Table 6. Although the two ranges of torque overlap, it is clear that the average torque developed during ballistic movements was substantially less than that produced during the force-matching experiment.

DISCUSSION

In Expt. I, subjects were found to be unable to match torques produced by different muscles with the elbow joint set at different angles. Their matching errors appear to be attributable to variations in the efficiency of the muscle–joint system, and the results are compatible with the hypothesis that subjects were matching muscle tensions. It is difficult to relate these results to those of Dern *et al.* (1947), Wilkie (1950) and Hunsicker & Greey (1957) because these authors used experimental situations quite different from that used in Expts. I–II and recorded the maximum forces that subjects could produce. However, Hunsicker & Greey (1957) report data for maximum forces which show that, as the elbow is flexed, maximum flexion forces increase more rapidly than maximum extension forces. This result is compatible with the matching data from Expts. I–II and supports the contention that matching errors were due to the varying relation between muscle tension and produced torque.

The wrist-grip used in Expts. I–II might have provided cues to produced torque. Clearly it did not, or the observed errors would not have appeared. The use of cutaneous-pressure cues was also rendered unlikely because of the high levels of pressure involved. Provins (1957) showed that for applied pressures greater than 250 g./cm^2 cutaneous receptors play no part in judging pressure. Of the 864 torques produced in Expts. I–II only three corresponded to a skin pressure less than 250 g./cm^2, and the effective skin pressure was greater than this, owing to compression of the skin by the wrist-grip.

The results of Expt. I imply that subjects cannot take into account variations in the mechanical efficiency of the effectors and cannot maintain constant effects on the environment by compensatory amplitude modulation. Since the pattern of results was unaffected by the speed with which subjects responded (i.e. ballistically or non-ballistically), it may be concluded that neither the motor nor the perceptual aspect of a motor-constancy system was operating.

It was predicted that since ballistic movements are the result of a sequence of ballistically applied torques, the pattern of error found when subjects attempt to match forces should also be found when subjects attempt to match ballistically produced movements. The results of Expt. II do not confirm this prediction. In general, movement matching was unaffected by the direction of movement of the forearm and the expected interaction between elbow angle and direction of movement did not appear. In other words, some mechanism intervenes which compensates for subjects' inability to match torques. Such a limited kind of motor constancy would be provided by a mechanism that compensated for varying effector efficiency by temporal modulation rather than amplitude modulation of the efferent signal to the musculature.

Partridge (1966) has presented evidence that, unless acting against near-infinite loads, muscle operates not as a simple force motor but rather as a complex system such that the input signal tends to act as a muscle-length demand signal rather than a tension signal. It might appear that such a system would provide a mechanism for movement constancy, although it would not operate effectively when isometric contractions are made. However, the same geometrical properties of the muscle–

joint system which cause mechanical advantage to vary with elbow angle would make the angular excursion of the forearm a non-linear function of change in muscle length. For constant angular changes to be produced under a variety of conditions, as they were in Expt. II, it would still be necessary for a central motor-constancy mechanism to operate.

Although a motor-constancy mechanism operating by temporal modulation is an attractive solution of the problems posed by these experimental results, it may be unnecessary. The levels of torque required to produce the movements in Expt. II were far lower than those produced in Expt. I. The possibility exists that while compensation can be made at low levels of torque, the compensatory mechanism breaks down at high levels of torque. With these reservations these experiments imply that while mechanical advantage and muscle length do not affect movement-matching accuracy, they do affect the accuracy with which torques can be matched. This discrepancy would be expected if there were a biologically valuable movement-constancy mechanism operating on the basis of temporal modulation of the efferent neural signal.

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THE ILLUSIONS AND GANZ'S THEORY OF CONTOUR DISPLACEMENTS

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Ganz's theory of contour displacements is summarized, and it is pointed out that its success or failure in explaining the illusions depends upon establishing the identity of the illusions and figural after-effects. Evidence is quoted which casts doubt on Ganz's claims for such an identity. Moreover, it is shown that predictions from Ganz's theory about some common illusions do not correspond with observed phenomena.

I. THE THEORY

Recently, Ganz (1964, 1966*a*, *b*) has developed the view held by Köhler & Wallach (1944) that figural after-effects (FAE) and a large number of the geometrical illusions depend upon the same mechanism. Both sets of phenomena involve a common principle of shifts in the apparent positions of adjacent contours. According to Ganz (1966*a*) all such contour displacements are due to the operation of lateral inhibition in the visual system, possibly in the retina. The neural correlate of a contour can be represented by a ridge of activity, the peak of which is assumed to determine the apparent position of the contour. The presence of a contour in the visual system is also assumed to give rise to lateral inhibition, and this inhibition is greatest at the locus of the contour, decreasing (probably exponentially) with distance on either side of the contour. If, then, two contours are adjacent in the visual field, inhibition from the one will decrease the activity due to the other, and this decrement will be greater for that part of the ridge of activity nearer to the inhibiting contour, so that the resultant peak of the ridge, and hence the apparent position of the contour, will be shifted away from the inhibiting contour. This is represented in Fig. 1. Of course, each contour will produce this effect on the other, so that each is, at one and the same time, both inhibiting and inhibited.

This analysis can be applied as it stands to the simultaneous illusions, but, in the case of the FAE there is a temporal separation between the interacting contours. In the typical FAE situation an inspection figure (IF) is fixated for a period, is then removed from the visual field, and a test figure (TF) is introduced in a position adjacent to the position of the IF, fixation being maintained. The apparent position of the TF is displaced from its objective position, the size and direction of the displacement depending on the spatial and temporal separation of IF and TF. In this situation, according to Ganz's theory, the lateral inhibition produced by IF is still present in the visual system after removal of the figure, and is responsible for the displacement of TF in the same way as when the two figures are simultaneously present, and is also responsible for the appearance of an after-image of the previously fixated IF. The FAE, then, is considered to be a simultaneous illusion between TF and the after-image of IF.

Ganz has presented some impressive evidence demonstrating that after-images and the FAE have very similar characteristics as regards such properties as intensity, onset, decay and interocular transfer. If the illusions and the FAE are, in fact, due to the same neural mechanisms, then they would, furthermore, be expected to exhibit the same properties, and the same figures presented both as simultaneous illusions and as FAEs should produce identical effects. Certainly, some of the work in this area which has been published seems to show a similarity between the two types

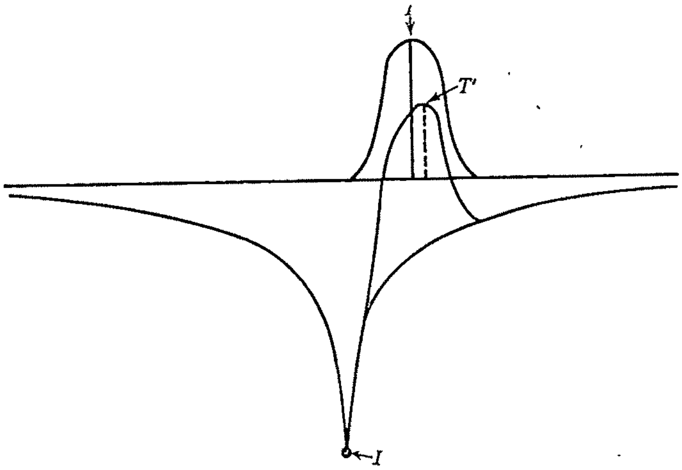

Fig. 1. Ganz's interpretation of contour displacements. Inhibition from the contour *I* (represented by the curves below the horizontal line) decreases the distribution of excitation due to contour *T*. The peak of the resultant distribution of excitation, *T'*, is the apparent location of contour *T*, and is further from *I* than is *T*.

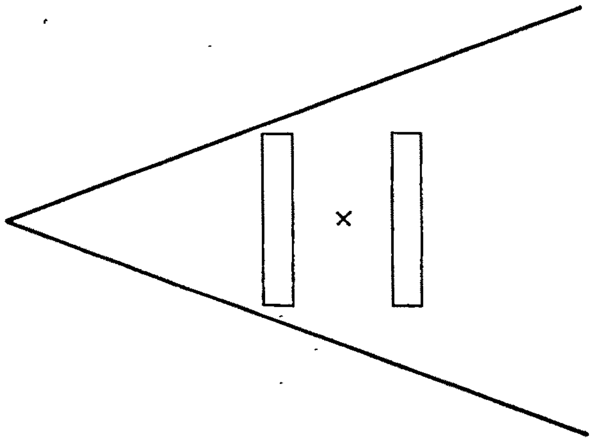

Fig. 2

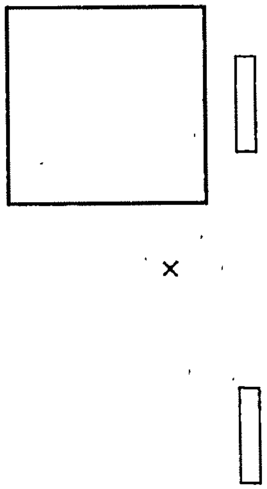

Fig. 3

Fig. 2. Presentation of the parts of this figure under FAE conditions produced the same effect as the normal, simultaneous Ponzo illusion, namely the left-hand rectangle appeared taller than the right-hand one (Köhler & Wallach, 1944). The cross is the fixation point.

Fig. 3. The displacement of the upper bar away from the square was found by Ganz (1964) to be the same when all parts of this figure were presented together, and when the parts of the figure were presented as an FAE. The lower bar is a comparison figure; the cross is a fixation point.

of displacement. Köhler & Wallach (1944) presented the Ponzo figure (see Fig. 2) to their subjects under FAE conditions, and the subjects reported the effect to be the same as the normal Ponzo illusion, when the whole figure is presented simultaneously. Ganz (1964) has shown that when the displacement of the upper bar in Fig. 3 is measured as a function of the separation of the bar and the square, the results are very similar when the whole figure is presented together, in the manner of an illusion, and when the square is presented as IF and the bars subsequently as TF and comparison-figure in the manner of an FAE. Spitz & Blackman (1958, 1959), working with retardates and normal subjects, have shown that those subjects who show a large FAE also tend to show a large Müller-Lyer illusion.

II. EVIDENCE AGAINST A SIMILARITY BETWEEN THE ILLUSIONS AND FAEs

To be set against the points just mentioned are a large number of investigations which show basic differences between the two types of displacements. One figure which has been extensively studied under both simultaneous and successive conditions of presentation is the Delboeuf illusion (two concentric circles). When this figure is presented as a simultaneous illusion, the inner circle appears *larger* than it really is, and this effect is greatest when the ratio of the diameters of the two circles is 3:2 (Morinaga, 1935; Ogasawara, 1952; reported in Sagara & Oyama, 1957). The same figure has been widely used in the investigation of the FAE, particularly by Japanese workers (see Sagara & Oyama, 1957). If the outer circle is fixated as IF and the inner circle subsequently as TF, it is found that the latter appears *smaller* than it really is, and this shrinkage effect is greatest when the ratio of the diameters of the circles is 2:1. Ikeda & Obonai (1955) presented this figure to their subjects with varying degrees of overlap of the presentation of the two parts, ranging from one extreme of complete simultaneity of presentation to the other of completely separate presentation. Their results showed an apparently smooth transition between the simultaneous illusion and the FAE types of displacement as the amount of overlap decreased. Ikeda & Obonai considered that this finding revealed a basic similarity between the illusion and the FAE. Ganz went further (1964, p. 466), and said that this result showed 'no essential difference between simultaneous and successively induced contour shifts'. It is not clear exactly what Ganz means by an 'essential difference', but it seems to the present writer that displacements in opposite directions, and with different spatial characteristics, are about as different as it would be possible to obtain.

More recently, Adam (1966) has reported a similar experiment to that of Ikeda & Obonai, in which subjects made two judgements of TF being larger or smaller than IF for each degree of overlap between IF and TF: one during the period when IF and TF were simultaneously present in the visual field, and the other after the removal of IF, when TF was still in the field. Under these conditions, Adam obtained two entirely separate sets of results, corresponding to the two sets of judgements, and which differed significantly in the absolute sizes of the displacements in the two conditions, and also in their development with decreasing overlap. Because of these differences, Adam concluded that Ikeda & Obonai's results, far from showing a gradual *transition* between illusion and FAE types of displacement, actually show a gradual *replacement* of the illusion by the after-effect.

Pollack (1964) showed that presentation of the parts of the Müller-Lyer illusion in the manner of an FAE reverses the direction of the illusion, and also it reverses the effect of age on the absolute size of the illusion (the normal illusion *decreases* with age, but the separately presented illusion *increases* with age). Similarly, Piaget & Lambercier (1944) have demonstrated that, while the normal Delboeuf illusion *decreases* with age, the FAE type of effect (the Usnadze effect) *increases* with age.

Summerfield & Miller (1955), using a version of the Zöllner figure, have illustrated another fundamental difference between the illusion and the FAE: the illusion was obtained both with free inspection and with fixation, but the FAE could only be obtained when the subjects fixated a single point on the figures. Another difficulty for Ganz's interpretation of the illusions is the effect of practice on the size of the illusions. It has repeatedly been shown (e.g. Judd, 1902; Judd & Courten, 1905; Köhler & Fishback, 1950*a*, *b*) that if a subject makes a large number of measurements of the size of an illusion, the illusion gradually disappears, and can even be made to occur in a direction opposite to that of the original illusion. To explain these findings Ganz's theory would require that lateral inhibition between the parts of the illusion figure can decrease, and even, in some manner, reverse its direction of effect. There seems to be no independent evidence that this can occur.

III. GANZ'S THEORY AND SOME COMMON ILLUSIONS

Ganz has not yet attempted to apply his theory of retinal inhibition to the illusions in any detail, although he claims that it will explain the Hering, Delboeuf, Müller-Lyer, Wundt, Zöllner, Poggendorff, Ponzo and Orbisson illusions (1964, p. 465; 1966*a*, p. 128; 1966*b*, p. 151). In fact, if we consider certain of these illusions in the light of Ganz's theory and of what is known about FAEs, we find that the effects observed, and the effects that the theory would have to predict, differ considerably.

The Müller-Lyer illusion. In the well-known Müller-Lyer illusion (see Fig. 4) the horizontal line of *A* appears shorter than the equally long horizontal line in *B* and shorter also than the plain line *C* of the same objective length. Here the processes in the visual system representing the 'arrow-heads' are presumably producing lateral inhibition, which inhibits the processes representing the horizontal line, and this results in an apparent displacement of the ends of the line inwards, away from the 'arrow-heads', in the manner described earlier. This displacement of the ends of the line towards its centre means that the line appears shorter than it really is. It seems clear that, if the 'arrow-heads' are producing a displacement of the ends of the line away from themselves in this way, a gap should appear between them and the ends of the horizontal line. This has not been reported in the extensive literature on this illusion, and, in informal observations carried out by the present writer on a number of people, both naive and sophisticated with regard to the illusion, a gap was not reported, even when specifically asked about, although the illusion was seen in every case. The predicted gap, were it in fact present, would be easily visible, as its size would be half the size of the illusion.

Now consider *B* in Fig. 4. The horizontal line here appears longer than that in *A*, and also longer than the plain line *C*. How does this come about? As far as

Ganz's theory is concerned, the situation here is hardly different from that in *A* of the figure. Here, also, we have 'arrow-heads' which, by the action of lateral inhibition, would be expected to cause displacement of the horizontal line inwards, away from themselves, with consequent apparent shortening of the line. Nothing that is known about contour inhibition, and nothing in Ganz's theory, suggests that inhibition acts on one side of the line but not on the other, and this would have to be the case if the theory were to explain why line *B* appears longer than line *C*. Of course, inhibition may well be greater in the acute angle of the 'arrow-heads' than it is around the point of the 'arrow-heads', and this could explain why *B* appears longer than *A*, but it cannot explain the fact that no gaps appear, nor that line *B* appears longer than *C*. In fact, the theory would have to predict just the opposite; that line *B* should appear *shorter* than *C*. To explain the apparent lengthening of *B* relative to *C*, Ganz would have to assume that, in *B*, the 'arrow-heads' are exerting, not an inhibitory, but a facilitatory action on the ends of the horizontal line.

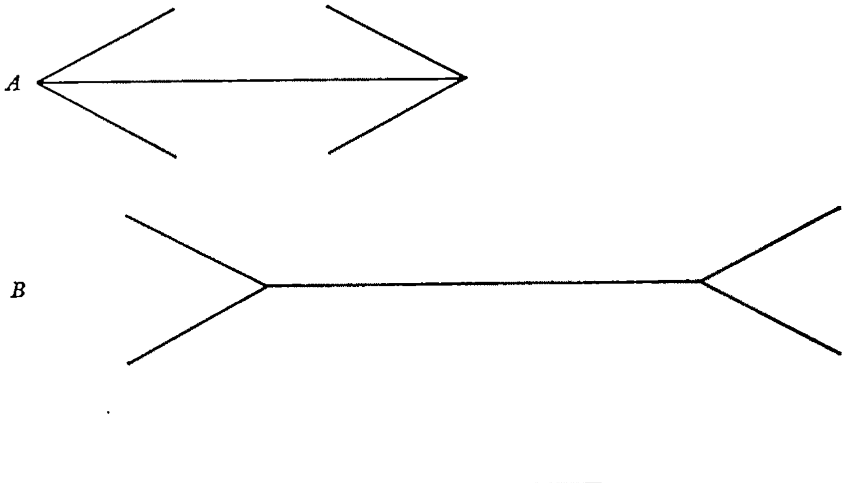

Fig. 4. Ganz's theory would have to predict the appearance of gaps between the 'arrow-heads' and the ends of the horizontal lines in the Muller-Lyer figures, and is unable to explain why line *B* appears longer than line *C*.

The Delboeuf illusion. Ganz's theory also has difficulty with the Delboeuf illusion which, as we have already seen, would be predicted to produce a shrinkage of the smaller circle, whereas what occurs is an expansion of the smaller circle.

The Ponzo illusion. Köhler & Wallach (1944), as we have seen, showed that the distortion of the Ponzo figure was qualitatively the same under both illusion and FAE conditions of presentation. According to Ganz's theory, however, the illusion, and, indeed, the FAE as reported by Köhler & Wallach's subjects, should appear only with certain restricted spatial relations between the parts of the figure. In the FAE the maximum displacement of the TF is not obtained when the figure is immediately adjacent to the location of the previously fixated IF, but at some greater separation, which is usually found to be that separation subtending a retinal angle of about 10–15 min. arc. This phenomenon is known as the 'distance paradox'.

It has also been shown that, at very small separations of IF and TF, attraction displacements may occur; that is, TF looks nearer to the position of IF than it really is. With these facts in mind, it can be seen that, in the FAE situation, where the angle of Fig. 2 is presented as IF, and the vertical bars as TF, the left-hand bar will appear longer than the right-hand bar only when the ends of the left-hand bar are at such a small distance from the lines forming the angle that attraction takes place, i.e. the ends of the bar attracted towards the lines of the angle, and an apparent lengthening will occur. The ends of the right-hand bar will necessarily be at a greater distance from the lines, and so, here, either a smaller attraction, or, more likely, a repulsion will occur, depending on the exact separation. The illusion could also occur in the FAE situation when the ends of the right-hand line are near the point of maximum displacement from the lines of the angle, the left-hand line being necessarily at a smaller separation where a smaller displacement will occur. In both cases the result is that the left-hand line appears longer than the right-hand one.

With the simultaneous illusion, the situation is somewhat different. In Ganz's theory the 'distance paradox' in FAEs arises because physiological nystagmus causes the retinal image of the TF to move over the retina relative to the fixed after-image of the IF. As the separation of IF and TF is made smaller, so the image of TF is actually on the opposite side of the IF for an increasing proportion of the time. When this happens, of course, displacement occurs in the opposite direction, and thus the net result is a smaller repulsion effect, which becomes smaller and smaller, and eventually may become an attraction, as the IF–TF separation is decreased. With the simultaneous illusion, nystagmus will not have this pronounced effect, because no part of the figure moves relative to any other part. Ganz predicts that here nystagmus must have some minimal effect, because contour inhibition appears to act with a definite latency (Kolers & Rosner, 1960), and so there will be a point of maximum displacement, which will, however, be at a point where IF and TF are almost exactly coincident. From this argument it can be seen that the normal Ponzo illusion should be almost impossible to obtain, as it should only occur when the ends of the left-hand bar in Fig. 2 are at such a minute distance from the lines of the angle that a decreased displacement will occur, the right-hand bar being at a greater distance, where a greater displacement will occur. It seems extremely unlikely that the numerous investigators of this illusion have always satisfied these rigorous conditions. Indeed, it is a simple matter to draw the Ponzo figure so that this condition is not met, by drawing the line that should be overestimated at a somewhat greater distance from the lines forming the angle than would produce a decreased displacement, and when this is done it is found that the illusion still appears.

IV. CONCLUSION

Ganz's theory of contour displacements seems, at present, to be rather more successful than preceding theories in the field of figural after-effects. The above discussion, however, suggests that it is not so successful when applied, as Ganz claims that it can be, to the simultaneous illusions. Perhaps a large part of this lack of success is due to the tendency of Ganz and other theorists to ignore the possibility that the illusions are not all due to the same processes. In this connexion, Robinson (1968) has recently suggested that the illusions can be fruitfully divided into two groups, which he calls 'distortions' and 'illusions'. He further suggests that Ganz's theory might well be applicable to the former group of figures (including the Poggendorff, Zöllner and Orbisson illusions) but not to the latter (which includes the Ponzo, Müller-Lyer and Delboeuf illusions). This possibility would be inconsistent with only small portions of the preceding discussion, and seems an approach worthy of further investigation.

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THE REVERSE MÜLLER-LYER ILLUSION AND 'ENCLOSURE'

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A reversal of the normal Müller-Lyer illusion occurs when relatively short lines are placed midway between the ingoing and the outgoing fins (Fellows, 1967). Gregory's (1967) explanation of this effect in terms of constancy scaling is unsatisfactory. An alternative hypothesis is proposed in which the reverse illusion is due to the enclosing effect of the ingoing fins. This hypothesis is supported by an experiment showing that an equivalent-sized non-fin enclosure (rectangle) has the same expansion effect upon a short line. The experiment also supported the argument that the reduction in the extent of the reverse illusion with increases in the length of the inter-fins line, and the eventual emergence of the normal illusion, are due to the progressive weakening of the enclosure effect and the strengthening of another effect specifically involving the pattern of the fins. The relation between enclosure and contrast is discussed.

A recent paper (Fellows, 1967) reported a reversal in the normal Müller-Lyer illusion when relatively short lines were placed midway between the ingoing and the outgoing fins. This finding was interpreted as failing to support Gregory's (1963) inappropriate constancy scaling theory and it was proposed that the reverse illusion may be due to the expansion effect upon the line between the ingoing fins, caused by the 'enclosing' nature of these fins.

In his reply Gregory (1967) made the following suggestion. 'If the heads correspond perceptually to the retinal projection of corners, a shorter line could represent, in the case of the outgoing arrows, some object nearer the observer than the extreme of the (inside) corner. In the real world this would give a larger retinal image than when placed at the corner; so to give constancy it must be shrunk with respect to the corner—which is what Fellows finds in his experiment' (Gregory, 1967, p. 222).

Two problems arise with this suggestion. First, as the previous experiment clearly showed, the *outgoing* fins have no significant effect upon the perceived length of a short line. Rather, the reversal of the Müller-Lyer illusion, at least with lines one half of the length of the gap between the fins, is entirely attributable to the expansion effect of the *ingoing* fins. Secondly, what could a short line between the ingoing fins represent? Surely, not some object farther away from the observer than the corner. But if, as seems reasonable, it represents something nearer, then by Gregory's argument it should also be shrunk—which it clearly is not.

Gregory (1967) did not accept the 'enclosure' explanation of the reverse Müller-Lyer illusion. As a further test of this hypothesis, an experiment was performed in which 26 subjects (college students) were asked to judge the relative lengths of enclosed and unenclosed lines. It was thought that, if the ingoing fins were replaced by an equivalent-sized non-fin enclosure, this would eliminate any effect due to the fins pattern, *per se*, but preserve the enclosure effect.

The new situation is illustrated in Fig. 1. The length of the rectangular enclosure is 160 mm, which is equal to the gap between the fins in the previous experiment (Fellows, 1967). The width of the line (10 mm) and the size of the cards (13 × 5 in.) were also the same as before. In order to determine the relation between the extent of the enclosure effect and the proportion of the length of the enclosed line to that of the enclosure, the whole range of line lengths from 20 to 140 mm was also used in this experiment. As before, PSE's for each standard length were determined by the method of constant stimuli (for details of stimuli and procedure, see Fellows, 1967).

Fig. 2 shows the required physical differences between the lines for PSE at the various standard lengths. The data from the present experiment (dashed line) are shown together with the results from the previous experiment (solid line). From this figure it can be seen that the expansion of the enclosed line was maximal at 80 mm, at which point the unenclosed line (line X) needed to be 2·7 mm longer than the enclosed line (line E) for PSE. In the previous experiment, at 80 mm the line between the outgoing fins (line O) needed to be 2·8 mm longer that the line between the ingoing fins (line I) for PSE. The equivalence of these two PSE's clearly supports the enclosure explanation of the reversal of the Müller-Lyer effect with short inter-fins lines. This implies that the pattern made by the fins plays no direct part in this effect, and hence that the expression 'reverse Müller-Lyer illusion' is a misnomer.

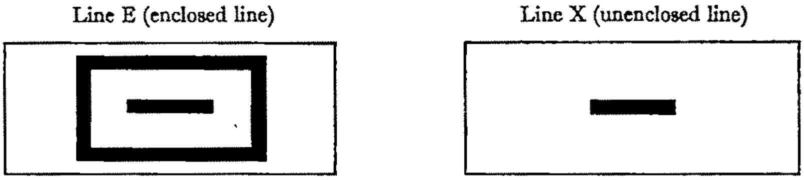

Fig. 1

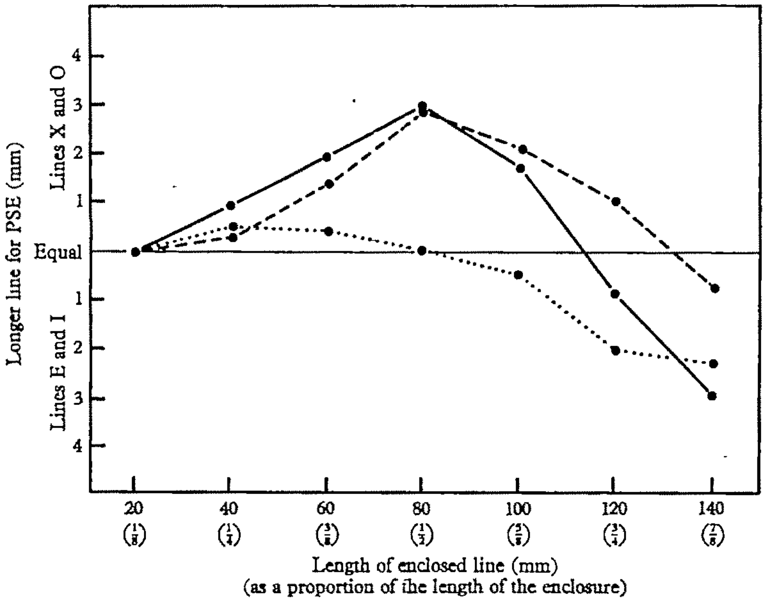

Fig. 2. ——, line I *v.* line O (Müller-Lyer figure); - - - - -, line E *v.* line X (enclosure effect); . . . . ., fins effect.

Fellows (1967) also argued that the reduction in the extent of the reverse illusion, and the eventual emergence of the normal Müller-Lyer effect as the length of the inter-fins line is increased relative to the gap between the fins (which remained constant), may be the result of the progressive weakening of the enclosure effect and the strengthening of the effect specifically due to the pattern of the fins. This argument is also supported by the present results. In Fig. 2, it can be seen that, as the length of the judged line increases above 80 mm, so the discrepancy between the enclosure effect (dashed line) and the Müller-Lyer effect (solid line) also increases. This increasing discrepancy most likely reflects the strengthening of the effect due to the pattern of the fins as the enclosed line approaches them. An attempt has been made in Fig. 2 (dotted line) to represent this effect by subtracting the enclosure effect from the Müller-Lyer effect.

One feature of the present data which does not fit too easily into the proposed theoretical framework is the reversal of the enclosure effect with the longest length of line. As shown in Fig. 2, at 140 mm the enclosed line (line E) needs to be 0·7 mm longer than the unenclosed line (line X) for PSE. This effect seems to be related to the Oppel illusions (Zanforlin, 1967), in which a line traversing an enclosure appears shorter than a physically equal isolated line. It seems possible that this effect is the result of the same process as that underlying the fins effect, though here the angle is 90°. If this were the case, then the extent of this reverse enclosure effect should be added to the fins effect (dotted line in Fig. 2) at 140 mm. This function, then, would be a more acceptable representation of the progressive growth in the strength of the fins effect as the inter-fins line approaches them than is depicted in Fig. 2. In any case, the shortening effect of 90° fins is damaging to Gregory's theory since it implies that some degree of outward pointing of the enclosing lines is necessary to maintain phenomenal equality with an isolated line.

The enclosure effect also plays a dominant role in the Delboeuf illusion, in which the inner of two concentric circles looks larger than a physically equal isolated circle. However, with this illusion, Piaget *et al.* (1942–3) reported an initial *contrast* effect: when the inner circle is relatively small compared with the outer circle (about a ratio of 1:4) then it appears smaller than the isolated circle. This contrast effect was not observed in the enclosure experiment reported above and may be specific to that type of situation in which there is an equivalence between the forms of the enclosed and the enclosing figures.

The distinction between enclosure and contrast can be illustrated by reference to the Ebbinghaus figure (Luckiesh, 1965, p. 56). In this illusion a circle which is enclosed by a ring of *smaller* circles appears larger than an unenclosed circle of the same physical size. Here both enclosure and contrast work together to expand the inner circle. However, if a circle is enclosed by a ring of *larger* circles the two illusion-producing forces work in opposite directions: enclosure expands the inner circle and contrast contracts it. The result of this is that the illusion, if any, is weak.

Cleary (personal communication) has suggested that the reverse Müller-Lyer illusion might be explained in terms of contrast. In his view (Cleary, 1966) geometrical illusions, such as Poggendorff, Müller-Lyer, divided line and Ponzo, are the result of three processes, which normally work to reduce binocular parallax anomalies in environments containing overlaid objects, operating inappropriately in the 2-D situation. Process 1 is said to shift contours intersecting at acute angles in the direction of that angle. In the Müller-Lyer figure, Process 1 will expand or contract, according to the fins, the line between the fins or; when there is no complete line, the space or the 'implicit contour' between the fins. Process 3, which is said to expand the length of elements of contours in proportion to the length of the contour of which they form part (the contrast effect), then acts upon the contour detail—the short inter-fins line—to give the reverse illusion. This, it seems, is a similar type of explanation to that of Gregory (1967). However, neither are satisfactory since they imply that the responsibility for the reverse illusion should be equally divided between the ingoing and the outgoing fins, which is not the case (Fellows, 1967).

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AN EXPERIMENTAL AND THEORETICAL APPRAISAL OF THE INAPPROPRIATE SIZE–DEPTH THEORIES OF ILLUSIONS

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Many explanations have been proposed to account for the distortions evident in illusions. Important examples of these suggest that the apparent size of a stimulus is determined by its apparent distance. The Müller-Lyer figure is frequently used to illustrate this principle. Problems are raised which make it difficult to explain the illusion in this way. Figures embodying depth features similar to those in the Müller-Lyer illusion are illustrated. These may be seen in depth and they will reverse in aspect also. If the 'inappropriate size–depth' theories are valid, illusory distortions should be seen in them. The apparent lengths of lines bounded by arrowheads are measured: they are inconsistent with the outcome expected if their sizes were determined from their apparent locations in depth. Detailed consideration is given to the implications of the findings for the inappropriate size–depth theories. Particular reference is made to the 'perspective', 'carpentered environment' and 'size-constancy' theories. It is concluded that these theories fail to explain why illusions appear in two-dimensional figures.

Many theories have been formulated in the attempt to explain why illusions appear in two-dimensional patterns. The most persuasive of these are based upon the influence of depth features implicit in such patterns. Thiéry (1896) noted the resemblances of some illusions to two-dimensional projections of scenes and structures. He drew particular attention to the Müller-Lyer (1889) figure, which reminded him of the cross-member and trestle-legs of a saw-bench. The possibility of depth being inferred in these figures, and of such inferences being responsible for modifying their apparent lengths, sizes and shapes, was developed in Thiéry's first systematic account of the 'perspective theory' of illusions.

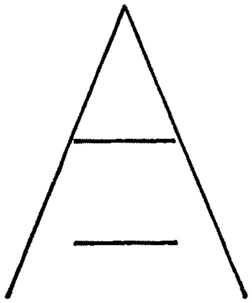

Fig. 1. The Ponzo illusion.

Closely related to the argument from perspective is the hypothesis of the 'carpentered world', which was developed from the early cross-cultural studies of Rivers (1901, 1905) by Segall *et al.* (1963, 1966). The casual properties of the perceptual mechanism invoked by the perspective theory are similar to those of the carpentered environment hypothesis. Hitherto, however, the possible ways in which any depth features implicit in two-dimensional patterns might be responsible for modifying their appearances have been stated in general terms only. It should be emphasized that the mechanism implied in all theories of this kind is understood to operate in two stages. First, certain figures are interpreted as giving the impression of depth. For example, in the Ponzo (1912) illusion shown in Fig. 1 the presence of the converging oblique lines serves to indicate increasing distance. Secondly, the sizes of any additional contours superimposed upon such patterns are judged in accordance with their apparent positions in depth. Thus the upper horizontal line of the Ponzo illusion appears farther away than the lower. When distance is judged in a flat figure in this way an illusion becomes apparent. In the Ponzo illusion the upper horizontal line appears farther away than the lower and this leads to its being judged longer. Since explanations of illusions expressed in these terms proceed to apparent size via apparent distance, they may be referred to as 'inappropriate size–depth' theories. The purpose of the present paper is to consider whether or not they succeed in providing a satisfactory explanation of illusory distortion.

EXPERIMENT I

The illusory pattern to which most research has been devoted was first described by Müller-Lyer. In this illusion a straight line, bounded and bisected by arrowheads, as in Fig. 2 (*a*), undergoes distortion so that its two halves appear unequal. The two components of the Müller-Lyer figure, shown in Fig. 2 (*b*), may be seen in depth and a number of attempts have been made to explain the illusion accordingly. It should be emphasized that all such explanations have referred to the two individual elements of this illusion. *They imply that the ends of the arrowheads are instrumental in setting the reference planes for distance against which the remaining features of the figures are seen.* Tausch (1954) also noted the resemblances of the Ponzo and Müller-Lyer illusions to three-dimensional forms. Subsequently, Gregory (1963, 1966*a*) proposed that one part of the Müller-Lyer figure represents a 'typical perspective view' of the outer edge of a building, while the other is strongly reminiscent of the inner corner of a room. Should perspective be instrumental in modifying the appearances of two-dimensional forms, a consistent explanation of these distortions might follow. If this were the case, the ends of the arrowheads marked X in Fig. 2 (*b*) would seem farther away in space than the shaft, while those indicated by the letter Y would seem nearer. Consequently, the shaft bounded by ingoing arrowheads would appear closer than the one bounded by outgoing arrows. It follows from Emmert's Law (1881) that *the apparently nearer line should seem shorter than that farther away*, and this characterizes the nature and direction of the distortions usually evident in these figures.

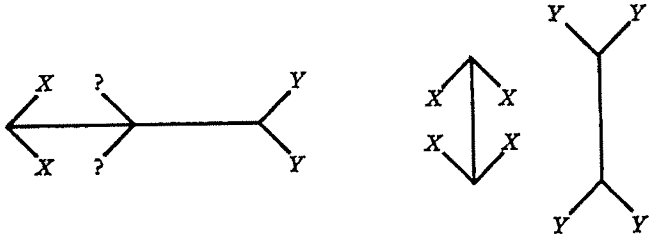

Fig. 2. The Müller-Lyer illusion in (*a*) composite and (*b*) component form.

All previous formulations of arguments of this kind are plausible; nevertheless, they are incomplete. If the explanation of the two parts of the Müller-Lyer illusion is valid, it should apply to the composite form also. An account of the complete illusion, however, has never been developed in accordance with any depth features which might be implicit in it. The reason for this omission is not difficult to determine. The arrowheads bounding Fig. 2 (*a*) could be interpreted as fixing the depth planes of the ends of the two halves of the shaft. This is indicated by inclusion of the letters *X* and *Y*, following the convention employed in Fig. 2 (*b*). *But what depth interpretation could possibly be placed upon the arrowhead which bisects the figure?* To maintain a consistent explanation in the case of the complete Müller-Lyer figure, the ends of this arrow must be seen as being *both in front of and behind* the shaft. If the illusion is to be explained in terms of apparent depth, the shaft must be seen simultaneously as both nearer to and farther from an observer. An interpretation of this kind seems highly questionable.

Method

The principal reason why the Müller-Lyer illusion has been studied so widely, particularly by adherents to inappropriate size–depth theories, is that adding arrowheads to straight lines gives a convincing depth impression, as illustrated in Figs. 3 (*a*) and (*b*). It is customary to measure the apparent lengths of figures of this kind in some convenient way and to compare them with such control patterns as are shown in Figs. 3 (*c*) and (*d*). There are, however, other ways of indicating depth about a straight line. One alternative is to add an arrowhead to each end, both pointing *in the same direction*. This is illustrated in Figs. 3 (*e*) and (*f*). Any depth features which these patterns may have are as equally compelling—if not more so—as those in Figs. 3 (*a*) and (*b*). If the argument from perspective is valid, the apparent lengths of the shafts of these figures should vary in accordance with their apparent depths.

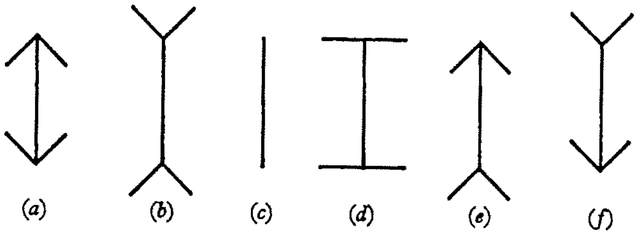

Fig. 3. The six patterns studied experimentally.

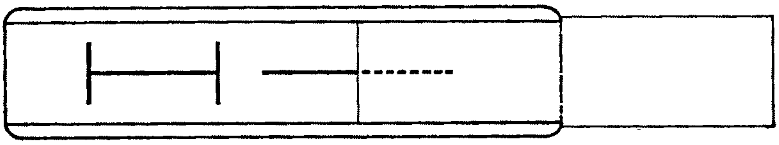

Fig. 4. A stimulus pattern alongside an adjustable comparison line.

Apparatus. Each of the six patterns illustrated in Fig. 3 was etched in black line upon a sheet of dense white plastic measuring 45 × 100 cm. The shafts were all 100 mm long, each arm of the bounding contours being 25 mm in length and all lines were 2 mm wide. An unbounded line, 150 mm long, was also etched on the plastic alongside the pattern. The distance between the adjacent ends of the shaft and line was 30 mm. Channels were fitted to the upper and lower edges of the plastic sheet to accommodate a slider, which could be moved across the face of the display so as partially to obscure the longer, unbracketed line. A machine-divided vernier scale engraved on the rear surface of the apparatus enabled the relative positions of the stimulus display and the slider to be determined with an accuracy of ± 0·1 mm. An example of this apparatus is illustrated in Fig. 4.

Procedure and subjects. The six displays were presented in random order to subjects, who were required to adjust the sliders so that the shafts of the figures and the comparison lines appeared to be of equal lengths. All subjects made four judgements of each display. The initial positions of the sliders and left–right orientation of the shafts and lines were randomized. The displays were orthogonal with the subjects' line of regard at a distance of 50 cm from the eyes. The verbal instructions given to subjects were as follows: 'I am interested to learn how well you are able to judge the length of one horizontal line with respect to another. Here is one such pair [display indicated]. Will you kindly adjust this slider [also indicated] so that both the horizontal lines appear to be identical in length?'

Twenty-three of the subjects were university students and the remaining 77 were sampled at random from the population of Newcastle upon Tyne. These latter were members of a panel formed with the intention of ensuring that those participating in a series of studies of perception should be representative of the populace. The subjects ranged in age from 17 to 64 yr., 57 members being female. The subjects were included in the sample irrespective of any visual defects they might have had: eight members wore spectacles whilst engaged in the experiment, but their judgements proved in no way atypical. The degree of sophistication of the subjects differed. Thirty members of the sample, all the students and seven members of the panel were familiar with perceptual phenomena of the kind under consideration. It is well-known that, in certain circumstances, experience of illusions modifies their extents. Data referring to 'experienced' and 'inexperienced' groups were compared, however, and they could not be distinguished statistically.

Results

The shafts of all the stimulus patterns were 100 mm long. The mean lengths of the comparison lines are shown in Table 1. The values ($\bar{X}$) refer to the combined judgements of the total sample of subjects and their standard errors of estimate ($\delta_{\bar{X}}$) are included also. The results indicate that the typical Müller-Lyer distortions appear in Figs. 3(*a*) and (*b*). They confirm the observations of Binet (1895), who found that the elongating influence of the outgoing arrows exceeds that of the foreshortening influence of the ingoing arrows. This difference remains to be explained. No significant distortion appears in Fig. 3(*c*). A slight degree of shortening appears in Fig. 3(*d*) and some small elongation is evident in both Figs. 3(*e*) and (*f*). For present purposes, the apparent lengths of the latter two patterns are most important. Small distortions appear in each of them, *but they are both in the same direction and they do not differ statistically.* The perspective elements in Figs. 3(*e*) and (*f*) are as strongly defined as those in Figs. 3(*a*) and (*b*). Hence it seems reasonable to propose that apparent differential depth *as such* cannot be held responsible for the spatial distortions evident in the Müller-Lyer illusion.

Table 1. *The apparent lengths* (*in mm*) *of the six experimental stimulus patterns*

(Letters refer to the patterns illustrated in Fig. 3.)

Display...	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)	(*f*)
$\bar{X}$	88·52	115·52	100·32	97·72	102·44	101·84
$\delta_{\bar{X}}$	1·46	1·17	1·18	1·16	1·24	1·42

EXPERIMENT II

The above argument rests upon depth features being indicated by arrowheads facing in the same direction. It might be noted that these patterns provide more appropriate control situations for evaluation of the Müller-Lyer illusion than have previously been employed. Figs. 2(*a*) and (*b*) show that the depth characteristics of two-dimensional patterns have been specified in a convenient but essentially *post hoc* way. It is not axiomatic that the ends of the arrowheads labelled X in Fig. 2 should appear to be situated at a farther distance than the shaft, nor that those marked Y should appear closer. Other interpretations are equally likely, even if it is accepted that these figures may be interpreted as projections of three-dimensional structures. If the ends of the *shafts* were to be seen as fixing the figures in depth, the barbs would appear to recede backward and to project forward respectively. But if this interpretation were entertained, it is difficult to see how any illusions could become apparent. Further, these figures can be interpreted in a way diametrically opposite to that assumed by the inappropriate-depth theories, since it is not difficult to see the relative positions of X and Y as reversed, i.e. X in advance of the shaft and Y behind it. Moreover, the two components of the Müller-Lyer figure will alternate readily in depth. It might be expected that the direction of illusory distortion would change upon reversal, but this fails to occur.

The depth features of Figs. 3(*e*) and (*f*) also alternate and it is a relatively simple matter for observers to maintain them in either of their two mutually exclusive aspects for short periods of time. The purpose of the second experiment was to determine whether or not the appearances of Figs. 3(*a*), (*b*), (*e*) and (*f*) are modified in accordance with the different depth aspects in which they may be seen.

Method and procedure

Only the operational details of this study need be described. The sample of subjects was the same as participated previously, and the four patterns illustrated in Figs. 3(*a*), (*b*), (*e*) and (*f*) were used again. The procedure adopted and judgements required were the same as before, except that some degree of sophistication was required of subjects. It was necessary for them to maintain the figures in different depth aspects whilst estimating their apparent lengths. To facilitate this, a drawing of the skeleton cube, first described by Necker (1832), was included in the letter with which an appointment was made with each subject. Along with this was a note describing the appearance of the cube in depth and a request to inspect it periodically before attending the laboratory. Particular reference was made to the two different depth interpretations of the Necker cube and to the need for maintaining it in either depth aspect for periods of ½ min. or so. Thus, before taking part in the study, the subjects had been familiarized with conditions similar to those required when carrying out the experiment.

All the subjects were instructed as follows: 'You recall how you were able to see the skeleton cube in two depth forms and how you found it possible to maintain it for a period of time in each of its aspects. These patterns can be seen in a similar way. In each case I will indicate that I wish you to "hold" the position of the shaft of the figure either in front of, or behind, the oblique lines. Thus the ends of the arrowheads should appear to recede backwards, or to project forwards, from the shaft. When you are satisfied that you can see the figure in this way kindly adjust the length of the comparison line so that it is the same as the shaft. Please take care in ensuring that the appearance of each bounded figure is maintained throughout the period during which you make any adjustments. Should it appear to reverse in depth just look away from the pattern momentarily and then repeat the procedure.'

Results

The mean lengths of the comparison lines are shown in Table 2 for both depth interpretations of the bounded shafts. As before, the typical Müller-Lyer distortions are evident in Figs. 3 (*a*) and (*b*), and again slight elongations appear in both Figs. 3 (*e*) and (*f*). The values of Student's statistic *t*, indicating whether or not the comparable sets of judgements were drawn from the same population of responses, are included in the table, which also includes estimates of the probabilities of the differences having occurred on the appropriate null hypothesis. Statistically, the judgements do not differ. Hence they appear to confirm that any depth features indicated by the oblique contours are not responsible for the illusion.

Table 2. *The apparent lengths of the four stimulus patterns maintained in both their depth aspects*

(Letters refer to the patterns illustrated in Fig. 3.)

Display	Shaft held	$\bar{X}$	$\delta_{\bar{X}}$	t	P
(*a*)	Forward	91·60	1·40	0·73	> 0·05
	Backward	93·20	1·68		
(*b*)	Forward	114·40	1·54	0·59	> 0·05
	Backward	113·20	1·36		
(*e*)	Forward	105·20	2·32	0·12	> 0·05
	Backward	105·60	2·58		
(*f*)	Forward	104·40	2·48	0·70	> 0·05
	Backward	106·80	2·40		

DISCUSSION

The problems posed by failure of certain figures to conform perceptually with their physical dimensions are simple to state, albeit difficult to solve. In everyday behaviour the spatial senses and the perceptual scaling mechanisms afford a veridical account of the physical environment. Certain of the transducer functions of the end-organs are reserved for encoding spatial information, whereupon the perceptual systems analyse it. Naturally, a characteristic degree of error is involved in these processes. Nevertheless, the accuracy with which judgements can be made and the degrees of sensorimotor skill which can be developed indicate that perceptual computations are usually reliable. Sometimes, however, remarkable distortions are evident; straight lines appear curved, regular forms misshapen and identical shapes different. Thus the encoding and analysing mechanisms which allow relatively accurate judgements to be made of the forms of some spatial patterns fail to do so when faced with others.

Since Oppel (1855) first drew attention to the 'geometrical optical illusions', many attempts have been made to explain them in terms of the structure and function of the eye and the visual projection system. The names of those involved in studying illusory distortions testify to the importance of developing an acceptable explanatory account of them. The arguments advanced against the majority of proposed explanations are well known and they require little further consideration. The theories which do merit detailed discussion are those which understand illusions to arise as a consequence of breakdown in the perceptual analysis of size in accordance with distance.

The 'face validity' of these theories is such that they are held widely by experimental psychologists. For this reason alone, it is timely that their basic postulates, their implications and their validity should be brought into question.

The principles of the perspective theory are fundamental to all inappropriate size–depth explanations. It rests upon the simple and persuasive assumption that the mechanisms responsible for perceiving three-dimensional space also operate when two-dimensional patterns are seen. Certain examples of these resemble front-, plan- and side-elevation projections of three-dimensional scenes and structures. Parts of these are usually located at different distances from the eye of an observer. If distance should be inferred from two-dimensional patterns and if size were to be determined accordingly, certain predictable distortions would appear.

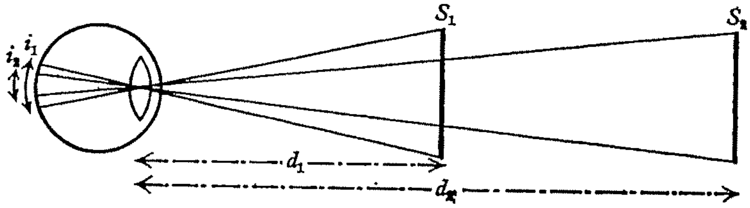

Fig. 5. Stimuli of the same length situated at different distances from the eye.

Hitherto, the only serious objection raised against the perspective theory was advanced by Gregory (1966*a*, *b*), who argued very convincingly that, while apparent distance is undoubtedly relevant to illusory distortions, the principles of perspective generate certain erroneous predictions. He illustrated these by reference to the distortion which characterizes the Ponzo illusion. Inspection of Fig. 1 reveals clearly that the *upper* horizontal line appears somewhat *longer* than the lower. Thus, in this illusion, *the line which appears farther away is elongated in relation to the one which appears nearer*. In the real world, a stimulus which is closer to the eye appears larger than one of the same size which is farther away. This is illustrated in Fig. 5. The retinal image i_1, due to stimulus S_1 at distance d_1, exceeds the size of image i_2 due to stimulus S_2 in the more distant position d_2. Therefore the distortion seen in the Ponzo illusion appears to conflict with the principles of perspective. Appealing to this relation between size and distance, Gregory drew attention to the contradiction which he considered the perspective theory to imply, stating (1966*a*, p. 147): 'Why should suggestion of greater distance produce increase in size when distant objects are typically seen as smaller with increasing distance? The theory predicts not an increase, but a decrease in the size of features having greater distance indicated by perspective, but this is the wrong way round.' That this is the principle upon which his criticism was based is confirmed by the further statement (1966*b*, p. 78): 'From this point the perspective theory goes off the rails. It states that depth is "suggested" by the perspective features, and this "suggestion" produces an expansion of the more distant features. But even if we allowed that a "suggestion" could cause a size change, it would be the wrong way round because distant objects in the real world look *smaller* not *larger*. In each case the traditional perspective theory gives exactly the wrong prediction.'

Gregory's critique is extremely plausible and it is widely accepted. Should it be tenable, it would imply that the explanatory principles of the perspective theory are incorrect. His contrary argument, however, rests upon the assumption that the two horizontal lines of the Ponzo figure are *actually* displaced by different distances from the eye. If this were the case their retinal analogues would differ correspondingly in size and the remainder of his argument would follow. But the two lines of the Ponzo illusion are not displaced in depth since, being drawn upon a two-dimensional surface, they lie in the same depth plane. Accordingly, the retinal images of the lines are similar in size. What does distinguish the two lines is their *apparent* rather than their *real* depths. Fig. 6 illustrates the relation between the apparent sizes of two stimuli, S_1 and S_2, which yield retinal images of the same size, i, but which are situated at different apparent distances, d_1 and d_2, from the eye. The stimulus which appears to be located at the farther distance seems *larger* in size that that which appears nearer. This is entirely in accordance with the distortion seen in the Ponzo figure. Hence the perspective theory cannot be confounded on these grounds. Gregory's criticism is mistaken, because it does not apply to situations in which retinal images are equated.

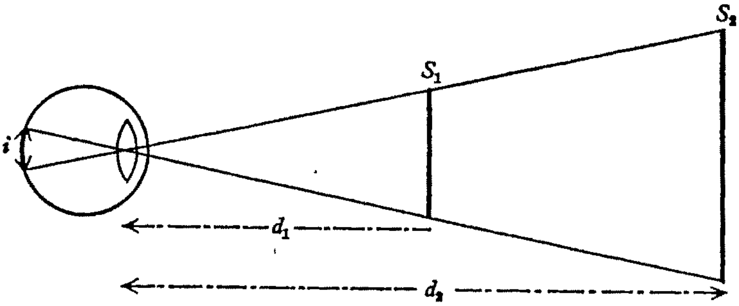

Fig. 6. Stimuli of different apparent lengths situated at different apparent distances.

Although the perspective theory can be defended against the criticisms levelled at it by previous writers, it carries a logical corollary which should be noted. According to the theory, the mechanisms, which allow three dimensions to be perceived accurately, operate inappropriately for two-dimensional displays. It follows that veridical perception of three-dimensional space is achieved at the cost of illusory distortion of two-dimensional space. Hence the argument implies that illusions are confined to two-dimensional patterns and that they fail to arise in three-dimensional scenes. This theme was developed by Piéron, who claimed that illusions disappear completely when they are given structural connotations. He attempted to relegate illusions to perceptual curiosities with little relevance to the broader problems of visual perception, observing (1955, p. 76) that 'if the two segments [of the oblique component of the Poggendorff illusion*] are made of rope passing behind a column and serving to pull a bucket out of a well, the illusion disappears completely. Consequently, I believe that one must not exaggerate the importance of optical illusions. I repeat, they have little connexion with everyday life.' He applied this argument to the Müller-Lyer illusion, which he also claimed fails to arise when embodied within a concrete context, concluding (1955, pp. 75–76) that 'the Müller-Lyer illusion is a phenomenon which never has any influence upon our spatial activity. It is a curiosity...the principle upon which the illusion is based is of no spatial importance.'

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* Poggendorff mentioned the illusion to Zöllner, who described it (Zöllner, 1860), but it was named for Poggendorff by Burmester (1896).

Piéron's observations and others carrying similar implications are directly relevant to the perspective theory, since they seek to capitalize upon its basic assumption. Empirical evidence for failure of illusions to persist in three dimensions was obtained by Filehne (1898) and by Ebbinghaus (1902). These studies have been repeated by Fisher & Lucas (1968) and by Lucas & Fisher (1968), who demonstrated that illusions remain when they form parts of three-dimensional structures and when they are embodied within two-dimensional patterns which depict real-life scenes. Hence it seems unreasonable to pursue the claim that illusory distortion of two-dimensional space is a necessary prerequisite for veridical perception of three-dimensional space.

The causal construct of the 'carpentered world' hypothesis is essentially the same as that of the perspective theory. Hence the above considerations are equally relevant to the view that familiarity of typical scenes and structures serves to develop appropriate habits of perceptual inference. Rivers (1901, 1905) and Segall *et al.* (1963, 1966) found that those living in different cultures differ in their susceptibility to illusions. Fisher (1968*a*) questioned the argument from the characteristic rectilinear nature of Western environments, however, in demonstrating that illusions continue to appear when their distorting elements are replaced by curvilinear contours. It can scarcely be denied that the ecologies of the primitive societies studied for these purposes contain many curved natural forms and a variety of circular artifacts. It should be emphasized that no dispute arises over whether or not illusions are seen differently by those who live in different environments. They almost certainly are. What is being brought into question is *why this should be the case.* Specifically under consideration is whether or not illusions depend upon different depth interpretations being made of two-dimensional spatial patterns. The hypothesis of the carpentered world appeals to the view that, *if* depth should be inferred, *then* illusions must arise as a consequence. Neither the present experiments nor the previous study referred to above can be interpreted as supporting such a proposition. Nevertheless, it should be pointed out that the appropriate comparison between the magnitudes of *rectilinear and curvilinear* illusions in *Western and non-Western* societies remains to be made. Preparations are now being made to make this comparison.

The points raised in the preceding discussion and the results of the experiments reported in this paper are also relevant to the contemporary theory of illusions based upon the inappropriate operation of the mechanism responsible for size-constancy. A detailed evaluation of this theory has been developed by the author elsewhere (1968*b*). The most convincing evidence cited in support of the size-constancy theory was obtained in a study in which remarkably high correlations were observed between the apparent lengths of the shafts of the two elements of the Müller-Lyer figure and their apparent locations in depth; i.e. the magnitude of the illusion was found to vary with apparent distance. From this study, Gregory (1966*b*, p. 95) concluded: 'The extent of the illusion follows the amount of recorded depth very closely, for both directions. We may regard this result as strong evidence for relating the illusion to perspective depth.' Had such patterns as those illustrated in Figs. 3 (*e*) and (*f*) been introduced into this study it would undoubtedly have proved possible to see them in depth. But their failure to exhibit different degrees of distortion would have made it difficult to draw a conclusion of this kind.

Gregory's interpretation of his experiment, however, is extremely relevant to theoretical discussion of the inappropriate size–depth theories. As was shown above, his argument against the perspective theory proceeded from mistaken assumptions. A further misunderstanding arose in his consideration of the relation between the magnitude of the Müller-Lyer illusion and the apparent location of its shaft in depth. The shafts of the two component parts of this figure are identical, but they *appear* to differ in length. Thus the shaft of Fig. 3 (*a*) seems shorter than that of Fig. 3 (*b*). The relation between the *apparent* lengths of stimuli and their *apparent* distances was determined by Emmert (1881) and it is expressed in the law which bears his name. Emmert's Law is frequently restricted to consideration of the apparent dimensions and locations of after-images, but it is applicable to any spatial judgement in which retinal image sizes are equal. The general case of the law is illustrated in Fig. 6. It shows that a line which appears longer also appears to be situated at a farther distance than one which seems shorter. A line bounded by outward-flaring arrowheads appears to be considerably longer than an otherwise similar line bracketed by inward-flaring arrows. On Emmert's Law, it is to be expected that contours differing in apparent length should also appear to be situated at different distances. *But such differences in apparent distance are irrelevant to explanation of differences in apparent size*, i.e. they fail to explain *why* the two lines should appear to differ in length *in the first place*. Thus the experiment intended to demonstrate the postulated relation *exploits the nature of the two component parts of the Müller-Lyer illusion as seen*. It fails entirely to indicate any mechanism which might be responsible for its appearance.

All versions of the inappropriate size–depth theories appeal to a two-stage perceptual process, according to which distance is determined first, whereupon size is judged accordingly. Innumerable studies of space perception have shown that the size of a visual stimulus can be interpreted from its apparent distance and, conversely, that its distance may be determined from its apparent size. These have served to reinforce the compelling view that an acceptable explanation of illusions is to be sought in terms of inappropriate perceptual computation of size and depth. But a question remains. Do the distance cues provided by such contours as the obliques of the Müller-Lyer and Ponzo figures induce illusions, or is *the presence of the contours themselves* responsible? The present experiments cast strong doubts upon the inappropriate size–depth theories, since apparent distance failed to determine apparent size. Accordingly, it is concluded that theories of this kind do not explain why illusions appear in two-dimensional figures.

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RELATIONS BETWEEN MOTION SICKNESS SUSCEPTIBILITY, THE SPIRAL AFTER-EFFECT AND LOUDNESS ESTIMATION

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This paper reports two experiments designed to elucidate those sensory characteristics which influence motion sickness susceptibility. In the first, a positive and significant correlation was obtained between the score derived from a biographical motion sickness questionnaire (MSQ) and the rate at which spiral after-effect persistence increased as a logarithmic function of the induction period (SAE slope score). A positive but insignificant relation was also found between the MSQ score and the slope of the sensation cupulogram. The second experiment revealed a significant concordance between the MSQ score, the SAE slope score and the slope of the psychophysical function describing individual loudness estimates of a 1000 c/s tone. The notion of 'receptivity' was advanced to account for these findings.

Motion sickness is a general term describing the ill-effects, principally nausea and vomiting, that accompany the perception of various types of periodic motion. Aside from the obvious practical implications of motion sickness, its theoretical interest for psychologists can be justified on the basis of, at least, two considerations. First, motion sickness is a truly functional disorder of the intact, healthy organism. That is to say, it occurs as a consequence of the normal processes of motion perception without the presence of any pathogenic agency, structural damage, or the excess or deficiency of any physical substance. Secondly, although empirical estimates of the general incidence of 'sickness' vary with the conditions of measurement, it is widely held (see Birren, 1949) that practically all individuals possessing an intact vestibular apparatus can be made 'sick' under suitable conditions. In a recent questionnaire survey (Reason, 1967), the present author found a positive history of motion sickness amongst 89·3 per cent of the undergraduate sample tested ($n = 300$).

The main concern of this paper is with the wide and relatively consistent differences between individuals in the extent to which they are susceptible to motion sickness. The two experiments reported here were designed to elucidate those primarily sensory characteristics which predispose some individuals to present the symptoms of motion sickness in conditions where others remain unaffected.

EXPERIMENT I

The first experiment was designed to test the hypothesis that motion sickness susceptibility (as measured by a biographical questionnaire) would be positively and significantly related to two measures derived from the spiral after-effect (SAE) phenomenon, namely (*a*) a persistence measure, and (*b*) a measure reflecting the slope of the plot: SAE persistence against log stimulus duration (induction period).

This hypothesis was based upon a number of experimental observations. Working independently, De Wit (1953), Mann & Canella (1956) and Preber (1958) found that a high degree of motion sickness susceptibility was associated with relatively persistent post-rotatory sensations following the impulsive stimulation of the non-auditory labyrinth. An additional finding was that, when the persistence of these labyrinthine after-sensations was plotted against the logarithm of the impulsive stimulation (i.e. the sensation cupulogram), highly susceptible individuals tended to be characterized by relatively steep slope values.

A recent correlational study (Reason & Benson, 1968) has shown that a positive and highly significant relation exists between the reported persistence of spiral and labyrinthine after-sensations. That is, individuals who tend to report relatively long spiral after-sensations also report long post-rotatory labyrinthine sensations (and conversely).

Finally, while it is well known that labyrinthine after-sensations increase as a logarithmic function of the impulsive stimulus (Van Egmond *et al.*, 1949), the present author has also found evidence to suggest that the reported persistence of the SAE shows a similar logarithmic relation to the 'objective' stimulus. Thus both the positive relation between spiral and labyrinthine after-sensation durations, and the similarity of their respective stimulus–response characteristics, suggest that there may be common features in the way inducing stimuli are translated into illusory sensations of movement by the two sensory systems.

On the basis of these observations, it seemed feasible to expect a relation between motion sickness susceptibility and the SAE similar to that previously demonstrated between susceptibility and the vestibular after-effect. The present experiment, designed to test this hypothesis, involved the administration of a personal history motion sickness questionnaire (MSQ) and the recording of spiral and labyrinthine after-sensation persistence following a graded series of 'objective' stimulus presentations.

Method

Tests and procedure

(i) *The motion sickness questionnaire* (*MSQ*). The questionnaire was of the conventional kind (see Birren, 1949). That is to say, each subject was asked to indicate how often he had felt nauseated and/or vomited whilst travelling on various types of vehicle (i.e. cars, buses or coaches, trains, aeroplanes, small boats, and ships) and funfair devices (i.e. swings, roundabouts, Big Dippers). An ordinal index of susceptibility was obtained by attaching numbers to the various categories of sickness frequency (i.e. never, rarely, sometimes, frequently and always) and then correcting these scores according to the individual's reported travel experience. The MSQ form was divided into two identical sections. The first section was devoted to the subject's experiences of motion sickness before the age of 12; and the second was concerned with his 'sickness' since the age of 12. An over-all index of motion sickness susceptibility was obtained by summing the separate scores from the two sections of the questionnaire.

(ii) *The SAE*. The stimulus material consisted of a four-throw, 180°, logarithmic spiral. The spiral disc was 8 in. in diameter and was placed at a distance of 5 ft. from the subject. Its speed of rotation was kept constant at 100 r.p.m. The subjects were instructed to maintain fixation on the brass centre-piece, and to say 'now' when they could no longer detect any apparent movement. The reported persistence of the after-sensation was recorded to the nearest 0·5 sec. using a stopwatch. Four values of prior stimulation (induction period) were used: 15, 30, 60 and 120 sec. Each stimulus value was presented four times in a randomized block design, and the order of presentation was different for each subject.

(iii) *Labyrinthine after-sensations*. The apparatus has been described elsewhere (Benson, 1962). The subject sat in a chair, which was mounted on a turntable, operated by a velocity servo-controlled electric motor. The subject's head was supported in the vertical position close to the axis of rotation by the use of a dental bite. His eyes were covered by opaque goggles and he was instructed to keep them closed.

The vestibular stimulus was an impulsive deceleration. The turntable was increased to the appropriate velocity at 1°/sec.[2] (a 'below threshold' rate of acceleration) and the speed held constant for a further minute. The turntable was then stopped by a hydraulically controlled braking system in approximately 0·5 sec. The subject was instructed to press a switch when he could no longer detect the after-sensation of apparent bodily rotation, and the duration of this after-sensation was read off from the recorder.

Four values of impulsive stimulation were used: 7·5, 15, 30 and 60°/sec. Each value of impulsive stimulation was given in both the clockwise and anti-clockwise directions of rotation, so that each subject received eight impulses in all.

The measures

(i) *Total after-sensation persistence.* For both the spiral and the labyrinthine after-sensations, a total persistence score was calculated for each subject. This was done by summing the mean after-sensation persistence values following each level of 'objective' stimulation.

(ii) *The slope scores.* For both types of after-sensation, a slope score was obtained from the time constant of the plot: after-sensation persistence against log stimulus value (either °/sec. or sec. of prior stimulation).

Subjects. The subjects were 48 male apprentices from the Royal Aircraft Establishment. Their average age was 17 yr., ranging from 16 to 20 yr.

Results

When the mean persistence values for both the spiral and labyrinthine after-sensations were plotted against the logarithm of the stimulus values (duration of stimulus for the spiral; impulse value for the labyrinth), the points in both cases were closely fitted by straight lines. Thus for both modalities the duration of the after-sensation increased as a logarithmic function of the 'objective' stimulus. These relations are shown in Figs. 1–2.

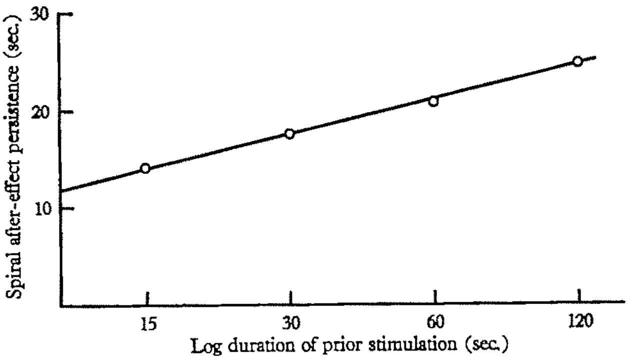

Fig. 1. The relation between spiral after-effect persistence and the duration of prior stimulation. Each point represents the mean of four values for each of 48 subjects. The straight line was fitted by the method of least squares.

The 'total persistence' and 'slope' measures for both after-sensations were correlated with the MSQ score, and the resulting product-moment coefficients are set out in Table 1.

In addition, a positive and significant correlation ($r = +0{\cdot}43$; $P < 0{\cdot}01$) was obtained between the persistence of the spiral and labyrinthine after-sensations. A positive, but insignificant, correlation was also obtained between the spiral and labyrinthine slope scores: $r = +0{\cdot}21$.

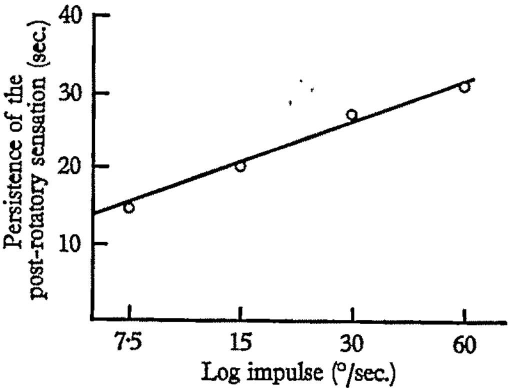

Fig. 2. The relation between the persistence of the post-rotatory sensations and the magnitude of the impulsive stimulus. Each point represents the mean of two values (one in the clockwise and one in the anti-clockwise direction of rotation) for each of 48 subjects. The straight line was fitted by the method of least squares.

Table 1. *Product-moment correlations between MSQ score and (a) the total persistence scores, and (b) the slope values*

	Correlation with MSQ score:	
	r	P
SAE persistence	+0·26	> 0·05
SAE slope value	+0·43	< 0·01
Labyrinthine persistence	+0·14	> 0·05
Labyrinthine slope value	−0·01	> 0·05

Discussion

From Table 1 it can be seen that the only measure significantly related to the MSQ was the SAE slope score. The direction of the correlation indicated that individuals with relatively steep slope plots tended to report a higher incidence of previous motion sickness than those with shallow or flat SAE slope plots. The surprising feature of these results was the fact that the relatively well-established relations between motion sickness susceptibility and the labyrinthine scores were not substantiated.

From these findings it seemed that a possible strategy for identifying the determinants of motion sickness susceptibility was to consider those factors which contribute to the individual differences observed in the SAE slope score. One interpretation that could be put on this slope variation was that it reflects characteristic individual differences in the way the central nervous system 'receives' or transduces stimulus intensity. In other words, the possibility exists that there are characteristic individual differences in the magnitude of the (neural) sensory response that is produced in relation to a given level of stimulus energy. This hypothetical source of interpersonal variation will subsequently be referred to as 'receptivity differences'.

It can also be suggested that those individuals who characteristically produce steep SAE slope plots, and who are relatively susceptible to motion sickness, may be those who tend to produce an idiosyncratically large sensory response to a given level of sensory input. In other words, it can be hypothesized that individuals with relatively steep spiral slope values were more 'receptive' to the intensity component of the inducing stimulation, and vice versa. This notion has some face validity for motion sickness susceptibility since it is those individuals with relatively steep SAE slope values (designated as more 'receptive') who tend to be the more prone. It seems reasonable to argue that if motion sickness depends upon the perception of a particular kind of motion stimulation, then individuals who perceive that stimulation as being more intense should also be more susceptible to its ill-effects.

The question that follows from this notion is: how else will these so-called 'receptivity differences' reveal themselves? From the evidence provided by Stevens (1957) it seemed likely that the slope (exponent) of the power function describing the relation between estimates of psychological magnitude and the physical magnitude of some 'prothetic' sensory continuum (e.g. brightness or loudness) would provide an indication of an individual's 'receptivity level'. This idea was based on the assumption that 'receptivity' is a general characteristic of the central nervous system rather than of any specific set of sense-receptors.

EXPERIMENT II

Following on from the arguments set out above, an experiment was carried out to test the hypothesis that both the MSQ score and the SAE slope value would be positively related to the slope of the psychophysical function describing loudness estimations. This particular sensory continuum was chosen because of its technical convenience, and because it has received considerable attention from previous workers (see Stevens, 1957). Theoretically, any 'prothetic' sensory continuum, involving the variation of stimulus energy, would have served as well.

Method

Tests and procedure

(i) *The MSQ.* The personal history questionnaire was similar to that used in Expt. I.

(ii) *The SAE.* The spiral was 5 ft. 5 in. in diameter, and the speed of rotation was kept constant at 70 r.p.m. Six durations of prior rotation were used: 15, 30, 45, 60, 75 and 90 sec. Each stimulus value was presented to the subjects twice, and the order of presentation was randomized. In all other respects the conditions of testing were identical to those used in Expt. I.

(iii) *Subjective estimations of loudness.* The simplest method of describing the test procedure is to quote the experimental instructions to the subjects verbatim.

'On each of the experimental trials, you will first hear a tone of standard loudness which we shall call the standard; then you will hear a tone whose loudness will vary from trial to trial, which we shall call the variable. The purpose of the experiment is to get some measure of how loud the variable tone seems to you in relation to the loudness of the standard.

'To obtain this measure, we will use the 100-point scale that you can see on the top page of your booklet. This will represent your subjective scale of loudness. To make the task simpler for you, we are going to call the loudness of the standard, 10. [A cross was marked on each scale at 10.] Your task is to estimate the loudness of the variable in relation to this standard, called 10. In other words, the question is: if the standard is called 10, what would you call the variable? Put a cross at whatever point along the scale that seems to you to give an appropriate indication of the loudness of the variable tone. All the variable tones will be louder than the standard.

'Please use one page of the booklet for each trial. During the trial, I will say "standard", and then give you the standard; then I will say "variable" and give you the variable.

'Try not to worry too much about being consistent. Just try hard to give the appropriate scale marking to each variable tone regardless of what you might have called the previous stimulus.'

The stimulus was a 1000 Hz tone and was generated from a 'Maico' clinical audiometer. The subject received each stimulus binaurally through earphones. The standard was set at 40 db (re: 0·0002 dyne/cm^2), and six variable stimuli were presented four times over in random order, paired with the standard. The values of these variable stimuli were: 50, 60, 70, 80, 90 and 100 db. Both the standard and the variable were presented to the subject for approximately 1–2 sec.

The Measures. When the SAE and loudness plots for each subject were examined individually, it was clear—particularly in the case of the SAE—that not all of these plots could legitimately be fitted by straight lines. Consequently, an 'estimated slope measure' was used that did not depend upon the assumptions of a logarithmic or a power relation, and which could be applied equally well to both the SAE and the loudness tests within the terms of the hypothesis. The slope measures for both tests were obtained as follows:

$$\text{estimated slope value} = B - A,$$

where A = the sum of the response values for the three lowest levels of stimulation, i.e. in the SAE, the sum of the mean persistence values following the 15, 30 and 45 sec. induction periods; in the loudness estimations, the sum of the 'psychological magnitudes' following the 50, 60 and 70 db sound pressure levels; B = the sum of the response values for the three highest levels of stimulation, i.e. the remaining stimuli in both tests.

Subjects. Forty subjects were used, eight of whom were female. The subjects included postgraduate and undergraduate students as well as academic and technical staff from the University of Leicester. All the subjects possessed normal hearing in the 1000 Hz range. Their average age was 24 yr. ranging from 17 to 35 yr. None had previously encountered the tests. A small payment was made to each subject.

Results

Spearman rank-order correlation coefficients were calculated between the MSQ score and the SAE and loudness estimated slope values. These are given in Table 2. These results indicate that there was a significant concordance amongst the experimental measures. That is, subjects reporting a relatively high incidence of motion sickness also tended to produce relatively steep spiral and loudness slope values.

Table 2. *Relations between MSQ score, SAE slope value and the loudness slope value* (n = 40)

	r_s	P
SAE slope/loudness slope	+0·53	< 0·01
SAE slope/MSQ score	+0·55	< 0·01
Loudness slope/MSQ score	+0·31	< 0·05

To illustrate these relations more clearly, the mean SAE plots and the median loudness functions for the 10 most 'sick' and the least 'sick' subjects are given in Figs. 3–4. The former were labelled the 'motion sick group' and the latter the 'non-motion sick group'. In Fig. 3 it can be seen that at all levels of prior stimulation the SAE persistence scores for the motion sick group were higher than those for the non-motion sick group; the differences (between the groups) becoming progressively more marked as the stimulus duration increased. The regression coefficients (b values) of the two plots were 10·98 for the 'sick' group, and 6·87 for the 'non-sick' group (where $Y = a+b \log X$). The slope differences in the median loudness functions of the 'sick' and 'non-sick' groups (see Fig. 4) were relatively small. The regression coefficients of these plots were: 'sick' = 0·016, 'non-sick' = 0·014 (where $\log Y = \log a + bX$). The extent of this slope difference was compatible with the relatively small correlation between MSQ score and loudness slope value ($r_s = 0{\cdot}31$).

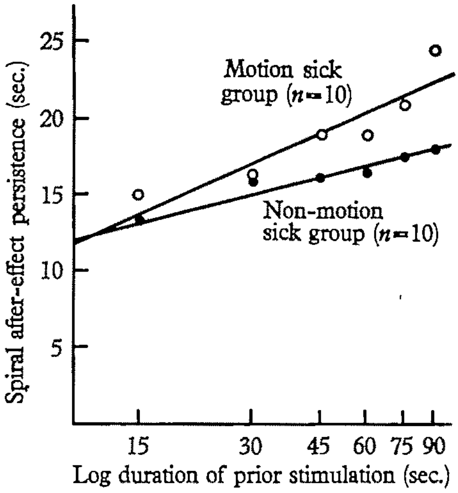

Fig. 3. A comparison of the mean after-effect persistence values for the 10 highest and the 10 lowest scorers on the motion sickness questionnaire (i.e. the 'motion sick' and 'non-motion sick' groups). The straight lines were fitted by the method of least squares.

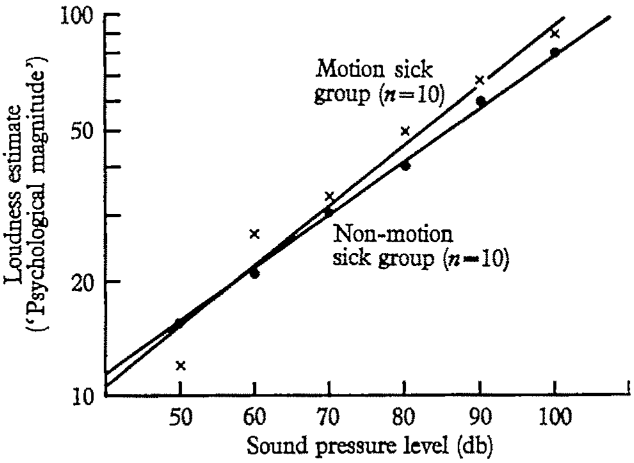

Fig. 4. A comparison of the median loudness estimates of the 10 highest and the 10 lowest scorers on the motion sickness questionnaire (i.e. the 'motion sick' and 'non-motion sick' groups). The straight lines were fitted by the method of least squares.

Discussion

The hypothesis that positive and significant relations would be found between the MSQ score and the SAE and loudness slope values was supported by the experimental findings. These results lent some credence to the notion that the interpersonal variation observed in all three of these measures reflected individual differences in the way stimulus energy is 'received' or transduced by the central nervous system. Thus 'receptive' individuals tend to produce steep SAE and loudness functions and to report a relatively high incidence of past sickness; whilst 'non-receptive' individuals show the oppositive characteristics. This proposed 'receptivity' typology provides grounds for a number of speculations.

In the first place, it is tempting to theorize that individual proneness to motion sickness is to some extent determined by the characteristic level of sensory response elicited by a given quantity of effective motion stimulation. This idea that susceptibility is linked to the idiosyncratic coding of motion intensity receives some support from the experimental observation (Alexander *et al.*, 1947) that, amongst a group of subjects, the incidence of 'sickness' increases as a function of the amplitude of the imposed wave-form. It can be argued, therefore, that highly susceptible individuals 'receive' sufficient stimulation to trigger the symptoms of motion sickness at relatively mild levels of intensity, whilst less susceptible individuals require greater physical intensity levels to evoke the same condition. Despite the fact that the notion of 'receptivity', in its present form, does not encompass the importance of stimulus structuring or informational content in producing proneness variability (Steele, 1963), it seems likely that the concept will prove of value in understanding the factors that determine motion-sickness susceptibility.

Another area in which the concept of 'receptivity' may prove useful is in accounting for the wide and relatively consistent individual differences that are observed in reporting the persistence of the SAE (Holland, 1965). Over the past two decades a large proportion of the studies involving the SAE have been directed towards demonstrating its effectiveness as a diagnostic and personality research tool. The justification for its use in these roles has been the assumption that interpersonal variation in after-effect duration can reveal pathological and idiosyncratic modes of cortical function. On the basis of the present findings, it can be suggested that part of this variation, at least, is explicable on the basis of 'receptivity differences'. Thus 'receptive' individuals tend to report longer after-effects than 'non-receptives' because, in them, a characteristically larger sensory response is evoked by the intensity (velocity) component of the inducing stimulus.

Finally, it is worth noting that the 'receptive–non-receptive' typology proposed in this paper is similar in many respects to three existing sensory typologies: (*a*) Teplov's 'strong–weak' nervous systems (see Gray, 1964); (*b*) Petrie's 'augmenters' and 'reducers' (Petrie *et al.*, 1963); and (*c*) Eysenck's introverts and extraverts (Eysenck, 1955). Although defined by different behavioural indices, and founded on somewhat different theoretical formulations, all four of these typologies share at least two important properties in common. First, they all postulate some characteristic variation in the processing of stimulus intensity. Secondly, the authors of these four typologies have all made empirical demonstration of the fact that these sensory characteristics are central rather than peripheral in origin. At a superficial level, it is feasible to suggest some identification between 'receptives', 'weak' nervous systems, 'augmenters' and introverts, on the one hand; and between 'non-receptives', 'strong' nervous systems, 'reducers' and extraverts, on the other. Some synthesis between the behavioural aspects of these dimensions has already been achieved at an empirical level (Gray, 1964). Further work along these lines would seem worth while in the hope that it will reveal a common source to these pervasive, and possibly significant, individual differences in sensory function.

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PHENOMENAL SIMULTANEITY AND THE PERCEPTUAL MOMENT HYPOTHESIS

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Successive brief visual stimuli falling within a critical time interval are phenomenally simultaneous. This paper examines two models of perceptual sampling which purport to account for phenomenal simultaneity. The first is Stroud's (1955) theory that the sensory input is quantized into successive, discrete summation periods or 'moments' (the Discrete Moment Hypothesis). An alternative model which has not generally been considered represents the 'moment' as a continuous, running sample of the input (the Travelling Moment Hypothesis).

Two experiments on phenomenal simultaneity are reported which provide a critical test between these two hypotheses. The results were entirely incompatible with the discrete moment model, which is therefore rejected. The travelling moment model accounted well for the results. These also suggest a possible relation between the limits of phenomenal simultaneity and the critical duration of brightness summation.

The suggestion has been put forward in many forms that, at some stage in the nervous system, the sensory input is packaged for analysis into successive, temporally discrete samples or 'chunks'. Underlying this suggestion is the idea that the brain operates in some way discontinuously in time on its inputs, an idea which will be referred to, for convenience, as the Perceptual Intermittency Hypothesis. This is to distinguish it from the related notion of intermittency in the selection of response, often referred to simply as 'central' intermittency, recently reviewed by Bertelson (1966). Perceptual intermittency has been proposed as an explanation for a remarkable variety of perceptual and other phenomena (Ansbacher, 1944; Boynton, 1961; Broadbent, 1958; Callaway & Alexander, 1960; Harter, 1967; Kinsbourne & Warrington, 1964; Kristofferson, 1967; Ladefoged & Broadbent, 1960; Lichtenstein, 1961; McReynolds, 1953; Michon, 1967; Murphree, 1954; Shallice, 1964; Stroud, 1955; White, 1963; Young & Stark, 1963; among others).

The hypothesis of perceptual intermittency envisaged by these writers has been coupled—sometimes implicitly—with an additional and much stronger postulate, namely that information regarding the temporal order of events falling within a given input sample is lost, e.g. as the result of a summation process. It is assumed, that is, that change in the input with respect to time can be represented only as *between* successive input samples, all events *within* a given sample being effectively simultaneous. The sample length thus becomes an irreducible unit, or quantum, of subjective duration—the perceptual moment. This much stronger postulate will be referred to as the Discrete Moment Hypothesis. Stroud (1955) provides its best known and most explicit formulation, but it is also assumed, explicitly or implicitly, by all of the writers listed above (with the possible exception of Broadbent, 1958) and in numerous other discussions of perceptual intermittency.

Historically the Discrete Moment Hypothesis has been linked with certain relatively specific suggestions about (*a*) periodic scansion as a basis for pattern recognition (Pitts & McCulloch, 1947), and (*b*) possible effects of cortical excitability cycles (e.g. Lindsley, 1952; see also the recent review by Harter, 1967). Logically, however, the

Discrete Moment Hypothesis is independent of any such speculations as to mechanism, and is considered here in its basic form. Certainly the evidence most often put forward in its support, concerning the temporal limits of interaction in the sensory input—and in particular, the span of phenomenal simultaneity between successive discriminable stimuli—has not been shown to relate to either (*a*) or (*b*) above.

In assessing the Discrete Moment Hypothesis two important points have been generally overlooked. First, it should be emphasized that loss of time-order information within the sample period is not entailed by the idea of discontinuity of operation (perceptual intermittency) *per se*. For instance, the handling of morse sequences in successive discrete 'chunks', as suggested in the original paper by Craik (1948), clearly presupposes the preservation of order information within each chunk. There is no contradiction here.

More importantly for the present argument, there is no reason *a priori* why the perceptual moment, defined as the span of subjective simultaneity, should be quantized in time. The logical alternative to this, which has not generally been considered, is that the 'moment', thus defined, corresponds to a continuously moving segment of the input function (the Travelling Moment Hypothesis). On this hypothesis all successive events in the input separated by less than the span of the travelling moment will be phenomenally simultaneous, although this relation, unlike the discrete case, is not transitive. In terms of information processing by the nervous system, the Travelling Moment Hypothesis requires a continuous, running sample of the sensory input, the basis for decision at any instant being some function, e.g. a moving average, of the input sampled over the previous $\frac{1}{10}$ sec. (or whatever the length of the moving sample happens to be).

A simple spatial analogy may help to make this distinction clear. To a man standing on the platform, the occupants of a passing train are revealed compartment by compartment as each window draws by. His glimpses of the interior of the train are essentially discontinuous in time. To an observer in one of the compartments, on the other hand, the field of view is always bounded by his own carriage window. New elements of the passing scene enter his view continuously from one side of the window, while others drop out of it at the other. Given a temporal rather than a spatial extension, the moving window analogy corresponds to the idea of a continuous 'travelling moment'.

The alternatives of temporally discrete *v.* continuous operation between them partition the set of possible models of input sampling in the nervous system, at least at any one level of perceptual analysis. (Consequently, experiments which could decide between these two broad classes of alternative would be optimally efficient in the sense outlined by Broadbent, 1956.) Yet very little of the evidence which has been put forward in support of a Discrete Moment Hypothesis has been even considered in the light of the continuous alternatives. To my knowledge the first published treatment of a travelling moment model, at least since James (1890), is due to Shallice (1964). In an illuminating theoretical paper Shallice considered four models of perceptual sampling derived from quality control procedures: two inherently continuous in operation and two discontinuous (including Stroud's discrete moment model). Unfortunately for a general decision on continuity *v.* discontinuity of perceptual sampling the results remained inconclusive.

Nearly all of Stroud's own evidence, for example, with the possible exception of the 'temporal numerosity' data (see White, 1963), can be accounted for equally well in terms of a travelling moment, where 'moment' is a period of temporal summation. Indeed some of the phenomena considered by Stroud—notably that of stroboscopic movement—could definitely not be predicted from his theory. According to the Discrete Moment Hypothesis, temporal interactions between successive events in the input should either occur or not occur in an all-or-none fashion, depending on whether the representation of these events overlaps in the same perceptual moment. The difficulty is that temporal interactions in perception are frequently not of an all-or-none type. The literature abounds with evidence to this effect. For a further discussion of Stroud's evidence, and of the other studies referred to at the beginning of this paper, see Allport (1966).

What is strikingly absent from the literature is any adequate experimental *test* of either the Discrete or the Travelling Moment Hypotheses, as distinct from the demonstration of phenomena compatible with them. The present paper is intended to fill this gap in the experimental literature.

GENERAL METHOD

Two experiments on visual phenomenal simultaneity, designed to provide a critical test between discrete and continuous (travelling) moment models, will be described.

Display. The display employed in both experiments consisted of an illuminated horizontal line which could occupy successively a sequence of 12 positions ('steps') on an oscilloscope screen, equally spaced one above the other. The position of the line changed instantaneously from one step to the next, and the whole sequence recycled continuously. The cycle time, and hence the rate of stepping, was controlled by the subject. In the simplest arrangement (Fig. 1*a*) the first step in the cycle followed immediately after the last, so that each position in this case was occupied for $\frac{1}{12}$ of the complete cycle time, τ. The line commenced either at the top and stepped downwards or at the bottom and stepped upwards.

Apparatus. The line patterns were displayed on the screen af a Cossor 1049 Mk. III oscilloscope. A cathode-ray tube containing a very short persistence 'blue' phosphor was employed. Rise and decay times of the trace were symmetrical with a time constant of *c.* 0·20 msec. The line was formed by a horizontal sweep on the x-axis internal time-base at 1·5 kc/sec., with trace suppression during flyback.

A voltage staircase was supplied to the y-plates of the cathode-ray tube by an Advance V.L.F. function generator SG 88. The central feature of this instrument is a glass disk, driven by a Velodyne motor, which rotates in the path of a narrow optical beam shining on to a photomultiplier. Any simple function blocked out in opaque material on the disk can thus be translated into a voltage as a function of time by the disk rotation. When running, the instrument emits a clearly audible whine, which changes in pitch as the rotation rate is altered. In order to prevent the use of auditory cues in setting a given repetition rate of the display, the subject wore a pair of well-padded headphones through which a masking noise of *c.* 60 db was presented. At this level subjects were unable to say with better than chance accuracy whether the function generator was running or not.

Stimulus arrangements. The subject observed the display binocularly from a distance of 6 ft., supported by a chin-rest. The lines on the cathode-ray tube screen were 2·5 in. long and sharply focused. Vertical separation between adjacent steps was constant at 0·18 in. (9′ of visual angle), giving a total height of 2 in. (1° 36′ visual angle). A curved white screen subtending 40° of visual angle concealed all but the cathode-ray tube screen itself, which was visible through a 2·5 in sq. aperture. Trace intensity was adjusted to 16 ft.-l. (100 ft.-l. in Expt. I, condition B). To determine the luminance of the trace the vertical separation between adjacent lines was reduced until they were contiguous, and Talbot brightness of the trace (S_1) recycling at 25 c/sec. measured with an S.E.I. photometer. Ambient room illumination was low—*c.* 0·01 ft.-l. at the reduction screen.

Subjects. Twelve university student volunteers, aged 18–25 yr., served as subjects for both experiments. They were tested individually, and were unaware of the purpose of the experiments.

Preliminary observations

At a very slow repetition rate subjects report typically a single horizontal line which appears to jump intermittently to a new position a little above (or below) its previous one. On decreasing the period of the display (i.e. increasing repetition rate) the subject sees not just one, but an increasing number of horizontal lines in apparent movement up (or down) the screen *en bloc* together. (The idea of using such an arrangement for measuring the limits of phenomenal simultaneity was suggested by Crawford in an unpublished study; Crawford, 1964.) As the number of perceptually 'simultaneous' lines becomes greater than half the total number of line positions (typically around 4–5 c/sec.) a figure-ground reversal occurs. The display is now more readily perceived as a stationary array of horizontal lines across which moves a dark band of shadow, temporarily obscuring some of them. This same phenomenon of 'shadow' movement is still readily observed with a display of only two line positions.

A further reduction in the total cycle time, τ, eliminates even the apparent motion of the 'shadow'. The entire display is now seen as a stationary array of lines *flickering apparently in synchrony*. A fine-grained 'trickling' effect, superimposed on the stationary lines, is reported by some subjects to persist up to the much *higher* frequencies at which flicker fusion occurs. Nevertheless the transition to apparent simultaneity (synchronous flicker) of all successive line elements in the repeating cycle, on abolition of the 'shadow' movement, is relatively abrupt. The period of the display, T_s, at which this occurs is taken as a measure of the span of simultaneity.

Increasing the vertical separation of the steps was found to increase T_s linearly. A preliminary experiment obtained results to this effect, paralleling data reported by Neuhaus (1929, tables 4–7) for a two-stimulus display, but with higher absolute values of T_s. This effect clearly has to be accounted for in any satisfactory model of phenomenal simultaneity. Fortunately it does not affect the predictions which these experiments were designed to test, and so will not be considered further here.

EXPERIMENT I

The effect of intra-cycle temporal pattern on the span of simultaneity

The span of phenomenal simultaneity, T_s, was determined under two intra-cycle temporal arrangements of the repeating 'staircase' display. Separate predictions were derived, and tested, from both the Discrete and Travelling Moment Hypotheses.

Method

The general method has already been given in a preceding section.

Stimulus arrangements. (1) Full cycle (S_1): the 12-step display recycled continuously, the first step in the sequence following immediately after the last. (2) Half-cycle ($S_{\frac{1}{2}}$): the sequence of 12 line positions occupied only half the complete cycle time. At the end of step 12 the cathode-ray tube beam was deflected off the screen for the remaining half-cycle, then recommenced at step 1 (see Fig. 1).

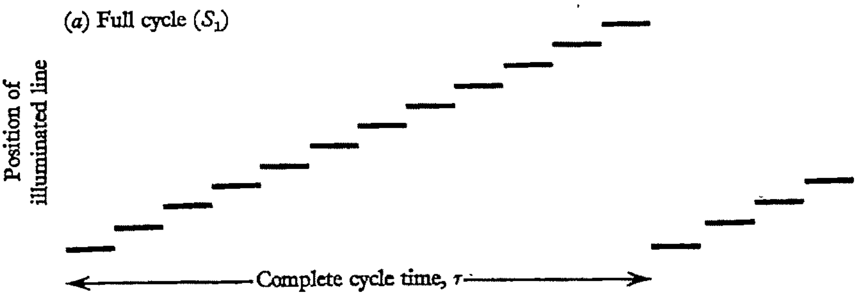
Two levels of trace intensity were used. In condition A (dim) the trace intensity was 16 ft.-l. In condition B (bright) it was increased to 100 ft.-l.

Design and procedure. The subject slowly adjusted the period (τ) of the display to phenomenal simultaneity, T_s, i.e. to the critical repetition rate at which all the lines just appeared to be flashing synchronously, with no indication of temporal differences between them. He was instructed to use as his principal criterion of simultaneity the absence of seen movement, either of the lines or of the 'shadow'.

Between each adjustment the experimenter set the period of the display alternately well below and well above T_s, and differing from it by a random amount. These will be referred to as ascending and descending trials respectively. On descending trials the subject was instructed to reduce τ slowly and carefully to simultaneity, going, if necessary, only very slightly beyond it and back again, until he found a point at which apparent movement was eliminated, but could be reintroduced by a very small increase in τ. On ascending trials he was similarly to increase τ slowly until the first signs of movement were detected, and again to 'bracket' the simultaneity point by as slight shifts of repetition rate as possible.

The subject was told to fixate within the area of the cathode-ray oscilloscope display, but no other control of eye movements was made.

Each subject performed four trials, two ascending and two descending, under each of the four stimulus conditions. The order of presentation was counterbalanced. Before the experimental trials about 10 min. were allowed for instructions and practice at the procedure, using the full-cycle arrangement (S_1) only. The subjects were unaware of the object of the experiment and of the nature of the temporal arrangements in the displays.


(*a*) Full cycle (S_1)

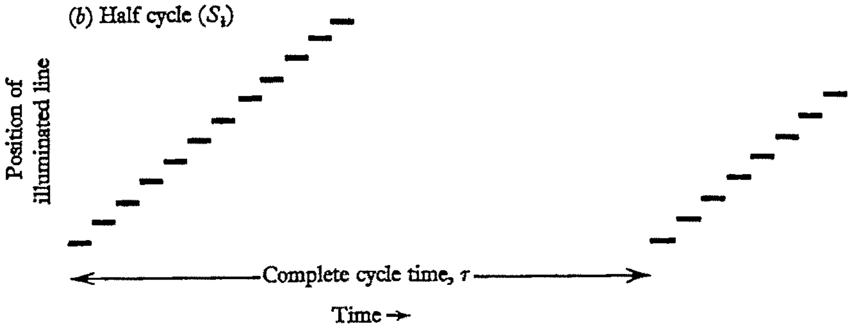

(*b*) Half cycle ($S_{\frac{1}{2}}$)

Fig. 1. Diagram of intra-cycle temporal arrangements for full- and half-cycle displays (Expt. I). Horizontal bars represent the proportion of the cycle for which each line position or 'step' is illuminated.

Predictions

1. *The Discrete Moment Hypothesis*

Ex hypothesi, when all the steps in the cycle fall within one discrete 'moment' (of duration M) they are perceived as simultaneous.

(*a*) *Synchronized 'moments'*. Given the possibility, envisaged by Stroud, of triggering the onset of moments t_0, t_1, t_2, ... to periodicities in the input, the Discrete Moment Hypothesis predicts the following results.

For the full-cycle display (S_1), simultaneity is reached on both ascending and descending trials when $\tau = M$ (Fig. 2*a*). For the half-cycle display ($S_{\frac{1}{2}}$), however, phenomenal simultaneity can be obtained *either* when $\tau = M$ *or* when $\tau = 2M$ (see Fig. 2*b*). Now on ascending trials the subject starts from 'simultaneity' (no apparent movement) and increases τ until movement reappears. This should first occur when

τ becomes just greater than M, i.e. when successive moments are no longer synchronized with each *complete* cycle of the display. The subject should therefore set $T_s = M$. On descending trials, on the other hand, simultaneity will first be reached when $\tau = 2M$, i.e. *when alternate moments are 'filled' and 'unfilled'*. Provided the subject follows the instructions and does not greatly overshoot this point, he will then set $T_s = 2M$. On descending trials, therefore, T_s for a half-cycle display should be twice that set for a full-cycle display (Fig. 2).

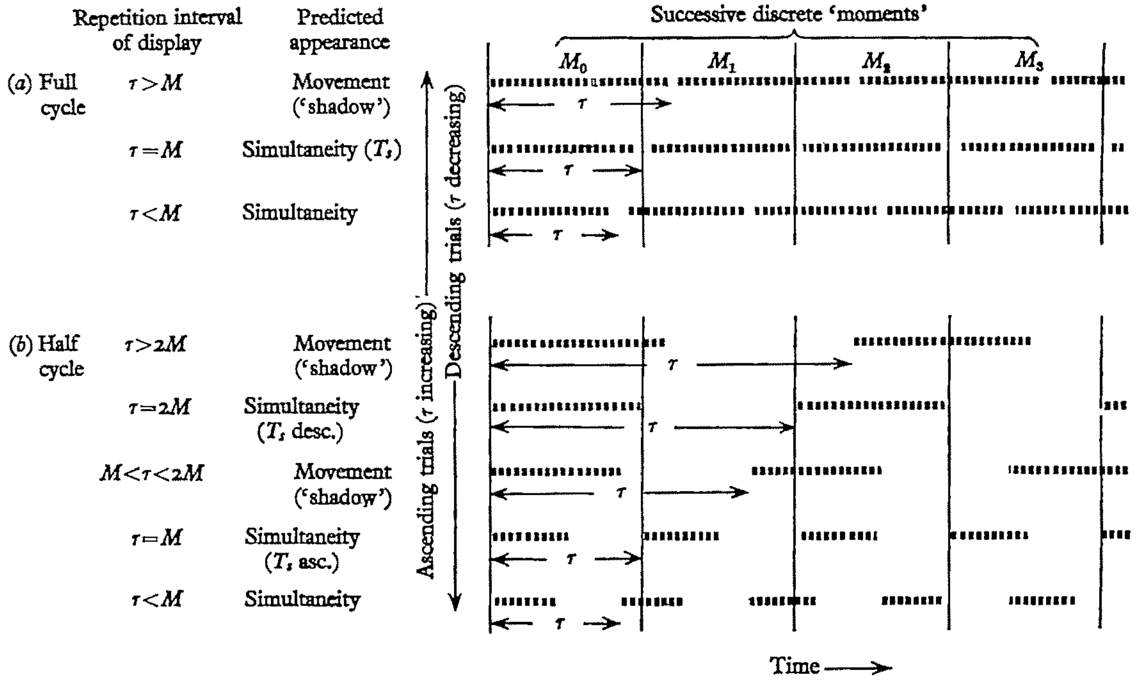


Fig. 2. Predictions from Stroud's Discrete Moment Hypothesis, (*a*) for the full-cycle display, S_1, and (*b*) for the half-cycle display, $S_{\frac{1}{2}}$, for both ascending and descending trials. Predictions assume the possibility of some synchronization of 'moments' with periodicities in the input (see text). Broken bars represent the time interval occupied by delivery of the 12 successive line positions ('steps') in the repeating display. Complete cycle time (repetition interval) of display = τ. Moment duration = M.

(*b*) If M were extremely variable, tending to synchronize with the input over a very wide range of τ, then 'bracketing' of T_s required by the instructions would be virtually impossible. M would remain locked to τ no matter how the subject varied the repetition rate. However, as indicated in the preliminary observations, the bracketing procedure was in fact quite easily carried out and yielded a relatively abrupt and stable value of T_s. Thus M must be assumed variable, if at all, only within narrow limits.

(*c*) *Self-paced moments*. Without Stroud's assumption of a facility for synchronization with the input (i.e. if M = const.) T_s should be reached, for *both* $S_{\frac{1}{2}}$ and S_1, when $\tau = M$. (With $S_{\frac{1}{2}}$ it might just be possible, on occasion, to set the half-cycle of the display in phase with the 'moment' sequence at $\tau = 2M$, although again, as for 1*a* above, this could only occur on descending trials.)

A much more definite and significant prediction regarding the half-cycle display is as follows: around both $\tau = M$ and (to a lesser extent) $\tau = 2M$ the period or half-period of the display should 'beat' slowly against the spontaneous 'moment' frequency, causing the apparent width of the 'shadow' to oscillate between zero and its maximum extent.

2. *The Continuous (Travelling) Moment Hypothesis*

According to this hypothesis successive sensory events will be perceived as simultaneous whenever their temporal separation is less than the extent of the travelling moment. They will, however, be perceived as simultaneous only *as long as* they are thus represented together within the moment span. In the present experiment, with a repeating sequence of stimuli, the criterion for T_s was not merely momentary simultaneity but *continued* absence of seen movement or any other temporal ordering. A subject obtaining an occasional momentary impression of simultaneity, immediately replaced by seen movement, would justifiably reduce the cycle time further until the apparent movement was eliminated. In other words, the criterion of simultaneity is met if and only if all successive elements in the repeating sequence are *continuously* present within the travelling moment span. That is, for *all* intra-cycle temporal arrangements, on both ascending and descending trials, when $\tau \geqslant M$.

Results

Ascending v. descending trials

The differences in T_s between ascending and descending trials, while statistically significant, did not exceed 4 msec. (Table 1). Contrary to the prediction from Stroud's Discrete Moment Hypothesis (1*a*, above), this difference was no larger for half- than for full-cycle displays (rather the reverse) and nowhere near the predicted ratio of 2:1. No individual settings even approach this ratio (see 1*c*, above). For the remainder of the analysis these differences were therefore regarded as negligible, and data from ascending and descending trials have been pooled.

Table 1. *Differences in the span of simultaneity, T_s, between ascending and descending trials*

Display	Intensity	T_s (desc.) − T_s (asc.) (msec.)	t	P
Full cycle	A (dim)	3·7	2·45	$< 0·05$
	B (bright)	3·9	3·31	$< 0·01$
Half cycle	A (dim)	4·1	2·70	$< 0·05$
	B (bright)	2·4	2·42	$< 0·05$

Effects of intra-cycle temporal pattern

The principal remaining results are summarized in Table 2. There are no very large differences in T_s between half- and full-cycle displays, but the latter required a consistently shorter cycle time to reach simultaneity at both intensities.

Effect of intensity

Between conditions A and B the oscilloscope trace intensity was increased by 0·8 log units, and this is reflected in a significant drop in T_s for both S_1 and

$S_{\frac{1}{2}}$ (see Table 2). Now in setting up the experiment, the trace brilliance was calibrated only for S_1. But for a given repetition rate the time spent at any one step in the half-cycle display, and hence the total light energy delivered during the cycle, is of course just half that for the full-cycle arrangement. Consequently the effective brightness of the $S_{\frac{1}{2}}$ was in fact 0·3 log units below the corresponding S_1 arrangements.

Table 2. *Critical cycle time, T_s, giving phenomenal simultaneity* (*msec.*)

Display	Intensity	Mean T_s	σ	Comparison	t
Full cycle (S_1)	A	86·2	12·5	S_1 *v.* $S_{\frac{1}{2}}$ (A)	3·45**
	B	70·7	8·1	S_1 *v.* $S_{\frac{1}{2}}$ (B)	2·92*
Half cycle ($S_{\frac{1}{2}}$)	A	95·5	16·0	S_1 A *v.* B	5·49***
	B	81·4	14·6	$S_{\frac{1}{2}}$ A *v.* B	2·44*

* $P < 0·05$. ** $P < 0·01$. *** $P < 0·001$.

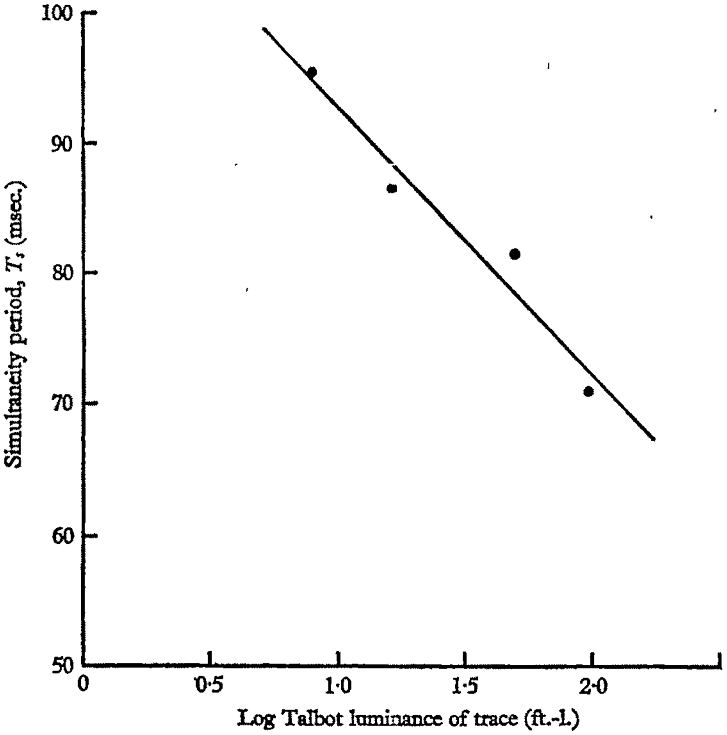


Fig. 3. Effect of trace intensity on the critical period of the display (repetition interval) giving phenomenal simultaneity between all successive line positions in the cycle.

The resulting brightness differences have been taken into account in Fig. 3. The straight line, fitted by the method of least squares, represents a decrease in T_s of 20 msec. per log unit increase in intensity. The degree of fit obtained suggests that trace intensity is in fact the single major determinant of T_s for all four stimulus arrangements.

Discussion

The results of this experiment entirely fail to support the predictions made from Stroud's Discrete Moment Hypothesis (1*a* above). Without the assumption of synchronization with a periodic input (1*c* above) the quantitative predictions are less clear-cut, and cannot be said to have been unambiguously falsified. Qualitatively, however, no effect which might be interpreted as 'beating' between the display and the hypothetical moment frequency (clearly predicted by 1*c*) was observed for any of the patterns on any occasion. These results, in the opinion of the writer, therefore oblige us to reject the notion of a discrete perceptual moment.

The travelling moment model, on the other hand, predicts no difference in the span of simultaneity between different intra-cycle temporal patterns. Where the effect of luminance is held constant (see Fig. 3), this prediction is substantially fulfilled.

In view of the remaining slight ambiguity of these results concerning the fixed-period discrete moment model, a second, and much simpler, experiment was carried out. The results of this second experiment were incompatible with any form of the Discrete Moment Hypothesis.

EXPERIMENT II

Direction of 'shadow' motion

In this simple experiment subjects were asked to report merely in which direction the 'shadow' appeared to be moving.

Method

Direction of stepping of the line, upwards or downwards, was randomized between trials, the direction on any trial being unknown to the subject. On each trial the cycle time of the display was increased from well below simultaneity (synchronous flicker of all 12 lines) until the subject reported clear movement of the 'shadow', in the form of a narrow dark band covering one or two lines, across the display. He was then asked in which direction the 'shadow' appeared to be moving. Some subjects described both a slow and a quick—almost instantaneous—phase of the shadow motion, in which case they were asked to report the direction of movement of the slow phase. Each subject performed four trials.

Predictions

Let the steps be numbered 1, 2, 3, ..., 12 in the order in which they occur within the cycle. According to the Discrete Moment Hypothesis, all the steps within the cycle will be phenomenally simultaneous when they are included within the same discrete 'moment'. When the moment length just includes, say, 11 of the 12 steps, the 'shadow' presumably represents the excluded ith line. Then in successive discrete moments the ith line will be replaced by line $i-1$, $i-2$, $i-3$, ... and the 'shadow' will appear to travel in the direction *opposite* to that of the sequence of line positions. This applies to all versions of the Discrete Moment Hypothesis. The situation can be illustrated as follows:

Steps...	8 9	1 2 3 4 5 6 7 8	9 1 2 3 4 5 6 7	8 9 1 2 3 4 5 6	7 8 9 1 2 3 4 5	6
Discrete moments		M_1	M_2	M_3	M_4	
Step excluded from moment		9	8	7	6	

Time →

Schematic representation of 'shadow' effect according to the Discrete Moment Hypothesis. (Diagram is for a display of nine steps.)

If, on the other hand, the span of simultaneity is determined by a continuous 'travelling moment', again including $n-1$ of the n steps at any instant, the excluded

ith line will be, successively, $i+1$, $i+2$, $i+3$, ... The reader should verify this statement for himself, if necessary, by visualizing the sequence of integers given above moving past a window which exposes $n-1$ at a time. Consequently the 'shadow' will appear to travel in the *same* direction as the sequence of line positions.

This observation is therefore crucial in distinguishing between temporally *continuous* and *discrete* models of the 'perceptual moment'.

Results and discussion

All subjects on all trials reported movement of the shadow in the *same* direction as the physical sequence of line positions in the display.

This result is incompatible with any form of the Discrete Perceptual Moment Hypothesis, which is therefore rejected. Other observations reinforce this general conclusion. For instance, at slower repetition rates of the display, when the 'shadow' appears to span more lines, it should also—on the basis of the Discrete Moment Hypothesis—appear to travel faster across the display. This was not observed. In fact, the reverse occurs, as the travelling moment model predicts. The results are of course entirely compatible with the Travelling Moment Hypothesis.

GENERAL DISCUSSION

The conventional account of phenomenal simultaneity and stroboscopic movement (e.g. Osgood, 1953) is based on some extension of Korte's 'laws' (Korte, 1915), which make no reference to the idea of a perceptual moment. Now whatever their precise form, an underlying assumption of such 'laws' is that it is the time interval between *individual* stimulus events (onsets) which is the critical variable. But in Expt. I it was found that simultaneity is reached at a constant *cycle* rate, regardless of individual stimulus intervals within the cycle. A similar finding has been reported by Lichtenstein (1961), who took it to support Stroud's Discrete Moment Hypothesis. However, the particular arrangements of his experiment precluded any discrimination between a discrete and a continuous (travelling) moment, with which his results were equally compatible. The results of the two experiments reported here show that the limit of simultaneity and apparent movement is determined by a process formally equivalent to a travelling moment—for which Korte's 'laws' may be seen to represent simply the special case with only two stimuli.

The discrimination of asimultaneity between spatially separate events (the subject-matter of these experiments) should of course not be confused with the detection of intermittency in any single visual location (flicker). The observations reported above were in any case all made at frequencies well below CFF with respect to the individual lines.

It has been pointed out (Kinsbourne & Warrington, 1964; Kahneman, 1967) that the span of simultaneity coincides with the temporal limits of a number of different types of interaction between stimuli, including backwards visual masking or metacontrast. The idea has also been advanced (Eriksen & Hoffman, 1963; Kahneman, 1965) that visual masking between successive stimuli may be at least partly accounted for in terms of temporal summation, resulting in reduced figure-ground contrast. In this connexion there is a remarkable similarity between the slope of T_s as a function of intensity (Expt. I, Fig. 3) and that found for the critical duration of visual brightness summation (e.g. Graham & Kemp, 1938; Keller, 1941; Biersdorf, 1955; Herrick, 1956). (Absolute values of intensity in Fig. 3 are based on the Talbot brightness of the cathode-ray tube trace itself. For comparison with the brightness summation data these should perhaps be averaged over the 1·5° height of the display, which would have the effect of bringing the absolute values into much closer agreement.) There is also evidence, in the case of the disk-ring suppression effect, that the critical interval for masking varies inversely with intensity in much the same way as in brightness summation (Holland, 1963). The similarity in each case may of course be fortuitous, but it is tempting and certainly economical to suppose, in view of the previous suggestions, that the temporal limits of these varieties of stimulus interaction depend on (or are matched to) some common underlying mechanism. If so, then Stroud may have been thus far correct in identifying the 'moment' with a period of temporal integration. His mistake was to ascribe it to a temporally discrete operation.

CONCLUSION

The experiments reported here have shown that the 'perceptual moment'—if by that is meant the span of simultaneity between successive visual stimuli—does not depend on a temporally discrete operation on the input. Consequently all forms of the Discrete Moment Hypothesis, including those theories which attempt to relate cortical excitability cycles, or periodic scansion, to subjective simultaneity (loss of time-order information in the input), must likewise be rejected. On the other hand, a continuous sampling process (the travelling moment model) accounts well for the results. If the arguments presented here are correct, then any remaining intermittency in the perceptual process, suggested for instance by the data obtained in temporal numerosity experiments, must be more central in origin than the phenomena of apparent movement, meta-contrast, and brightness summation most often described in the terminology of the perceptual moment. *A fortiori*, evidence of the temporal characteristics of these effects can have little relevance to the hypothesis of perceptual intermittency, which must look elsewhere for its empirical support.

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TWO-FLASH FUSION THRESHOLD: THE INFLUENCE OF AGE, PSYCHOPHYSICAL METHOD, INSTRUCTIONS, VIEWING CONDITIONS, SEX AND SUBJECT VARIABILITY

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In a series of studies with normals concerned with the time interval requred for two successive light flashes to be perceived as one flash (TFF threshold) it was found that: age and psychophysical method are related significantly to the TFF score; binocular viewing requires a shorter inter-flash interval for fusion than monocular viewing; handedness has no significant relation to monocular score; sex and instructions are not significant variables, but results suggest the importance of strict experimental control. Age is also related to vacillation around the fusion point. The data indicate that theoretical excursions with the psychopathology of TFF could be premature.

The fusion phenomenon for successive visual stimuli has been studied in recent years by presenting the subject with two successive flashes of light with varying inter-flash intervals (IFI). Under these conditions the longest IFI at which the subject fails to discriminate the two light flashes is known as the two-flash fusion threshold (TFF) and is measured in milliseconds.

Current interest in TFF is centred largely on the effects of induced stress (Roth, 1961) and the hypothesized relation of TFF to arousal level and schizophrenia (Venables & Wing, 1962; Venables, 1963*a*, *b*; Horn & Venables, 1964; King, 1962). Most workers in the area relate the theory to the results of Lindsley's study of the fusion threshold as indicated by the cortical responses shown on the EEG of the anaesthesized cat and the effects of reticular stimulation on the fusion threshold (Lindsley, 1957).

This report is part of a series primarily concerned with the pathological implications of the change in TFF. However, the present writers consider that theoretical excursions into the psychopathology of TFF are premature until much more is known of the influence of common variables which could affect the TFF score. This is of particular importance as some of the group differences in scores reported in the pathological studies have been of very small magnitude indeed and could possibly be accounted for by sampling issues. This argument is supported by the conflicting results of the very extensive research into critical flicker frequency threshold (CFF).

It has been suggested that there is no relation between CFF scores and TFF scores (King, 1962); nevertheless, one would expect that variables known to affect CFF could also influence TFF. The following experiments are concerned with the effects of age, psychophysical method, instructions, handedness, viewing conditions, sex and subject variability with normals.

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METHOD

Subjects. These were from the general community, being largely school children and their parents. They were informed that the test was one for visual acuity. Those with visual defects were excluded, as were those of known emotional instability or recovering from a recent illness.

Apparatus. There were two main components, namely a cathode-ray tube and a tuning device, producing square wave flashes 5 msec. in duration. The visual angle was 26·86°. Dial tuning permitted discrete settings of 1 msec. throughout the working range, i.e. from 10 to 199 msec. In front of the tube a dim filament with external control provided a fixation point. When seated in front of the apparatus the subject perceived either one flash of light or two successive flashes on the display screen. Each flash was 50 ft.-lamb. in intensity.

Two light flashes were presented by the experimenter for each trial but the inter-flash interval varied according to experimental requirements. Each subject was in the testing situation for about 15 min. and received about 140 consecutive trials.

Instructions

One of two types of instruction, chosen to create two types of set, were read aloud according to the investigation. These were:

Instruction 1 (sometimes one flash, sometimes two). 'Sometimes you will be shown two flashes and sometimes one flash. Say "one" when you see one flash and say "two" if you see two flashes. If you are not sure, don't guess. Say "not sure".'

Instruction 2 (always two flashes). 'You will be shown two flashes each time. Sometimes you will see the two flashes, but sometimes the flashes will be so close together that you will see them as only one flash. Say "one" if you see one flash and say "two" if you see two flashes. If you are not sure, don't guess. Say "not sure".'

Psychophysical method

The following two methods were used to determine TFF threshold.

Method A. This was the same as the modified constant method used by Venables & Wing (1962). A pair of flashes with a long IFI were presented. Following the response 'two', flashes with a very short IFI were given to elicit the response 'one'. The procedure was repeated, progressively reducing the long IFI and lengthening the short IFI, until two IFIs, 1 msec. apart, were reached. Flashes at these IFIs were then repeated twice, and if the responses were maintained the IFI at which the response was 'one' was taken as the threshold.

Method B. This was a variation of the method of limits used by Roth (1961). The procedure was as follows: (1) An orientation trial was given, using method A above. This trial provided an approximate threshold. (2) Commencing from 20 msec. above the approximate threshold, the IFI was reduced by 1 msec. intervals to 20 msec. below the point of fusion. The procedure was then repeated in reverse. Each interval was presented once only unless the response was 'not sure', when the interval would be repeated. (3) Stimulus pairs were initiated every 5 sec. (4) One descending and one ascending trial only. (5) The TFF threshold was computed by taking the mean of the point at which the subject first responded with 'one' on the descending trials and the point at which he last responded with 'one' on the ascending trials. (Intra-subject variability is discussed below.)

General testing procedure

The subject sat at a desk facing the display panel, with the head in a chin rest maintaining the eyes in line with the optical axis of the apparatus and 4 ft. from the stimulus source. All testing was in dim ambient light conditions, varying between 0·01 and 0·02 ft.-candles.

Two light flashes were presented by the experimenter for each trial, but the inter-flash interval varied according to experimental requirements. Each subject was in the testing situation for about 15 min. and received about 140 consecutive trials.

INVESTIGATION 1

Age, instructions and method

Subjects. These were 60 right-handed females, aged from 5 to 50 yr. In 23 cases mothers and daughters in the same family were tested. They were classified into 10 age-groups with six people per group as follows: 5–6, 6–7, 7–8, 8–9, 9–10, 14–15, 16–21, 22–31, 32–41 and 42–50 yr. of age.

Instructions. The six subjects at each age level were divided at random into two groups: one subgroup was read instruction 1 (sometimes one flash, sometimes two flashes) and the other subgroup received instruction 2 (always two flashes).

Method. All subjects were tested with the two methods described above, with the order for commencing each series of trials being randomized within each subgroup at all age levels. Only binocular viewing was used.

Statistical design. To test for main effects as well as interaction effects, a three-factor analysis of variance was carried out on the data (Winer, 1962, case II). This gave a 10 × 2 × 2 design.

Results

The analysis of variance summarized in Table 1 shows that age and method are significant variables, but instructions is not. There are no significant interaction effects. Table 2 is a summary of mean TFF thresholds for the three main effects. The indications are that the IFI for fusion tends to be greater (*a*) for older subjects, (*b*) for method A, and (*c*) for instruction 1. Table 3 shows Spearman ρ correlations

Table 1. *Three-factor analysis of variance of TFF thresholds obtained from 60 right-handed female subjects aged 5–50 yr.*

Source of variance	S.S.	D.F.	M.S.	*F*	*P*
Between subjects	19073	59			
Age	5676	9	630·7	2·294	< 0·05
Instructions	915	1	915·0	3·328	
Age × instructions	1483	9	164·7	0·599	
Error (A)	10999	40	274·9		
Within subjects	369	60			
Method	28	1	28·0	4·828	< 0·05
Age × method	42	9	4·7	0·810	
Instructions × method	2	1	2·0	0·345	
Instructions × age × method	63	9	7·0	1·207	
Error (B)	234	40	5·8		

Table 2. *Summary of mean TFF thresholds for age, method and instructions*

(Scores in msec.)

	Age (yr.)										
	5–6	6–7	7–8	8–9	9–10	14–15	16–21	22–31	32–41	42–50	Mean
Method A											
Instruction 1	31	48	54	56	51	53	57	60	53	60	52·3
Instruction 2	36	50	51	43	48	44	60	44	47	61	48·4
Mean	33·5	49	52·5	49·5	49·5	48·5	58·5	52	50	60·5	50·35
Method B											
Instruction 1	34	46	55	58	52	54	62	64	54	61	54·00
Instruction 2	36	51	49	45	46	46	62	45	51	59	49·00
Mean	35	48·5	52	51·5	49	50	62	54·5	52·5	60	51·50
Mean age	5·5	6·4	7·4	8·4	9·4	15·0	20·5	29·8	36·7	46·3	

for TFF thresholds with age for both method and instruction. Significant results exist for methods A and B under conditions for instruction 1 only.

Inspection of Table 2 suggests that differences in instruction are not meaningful to children of 7 yr. of age and under. The over-all age effect is an opposite trend to that obtained by Mesiak (1947) and Cross (1963) for CFF, supporting the suggestion by King (1962) that CFF and TFF measure different functions.

Table 3. *Spearman ρ correlations of TFF threshold and age* ($n = 10$)

	Instruction	ρ	P
Method A	1	0·71	0·05
	2	0·38	n.s.
Method B	1	0·66	0·05
	2	0·48	n.s.

Table 4. *Three-factor analysis of variance of TFF thresholds obtained from* 15 *right-handed male subjects and* 15 *left-handed male subjects to investigate the variables of handedness, method of obtaining threshold and variations in viewing conditions*

Source of variance	S.S.	D.F.	M.S.	F	P
Between subjects	15455	29			
Handedness	985	1	985	1·9	
Error (A)	14470	28	517		
Within subjects	3565	150			
Method	353	1	353	63·4	< 0·001
Handedness × method	0	1	0	0	
Error (B)	156	28	5·57		
Presentation	439	2	219·5	6·01	< 0·01
Handedness × presentation	78	2	39	1·07	
Error (C)	2044	56	36·5		
Method × presentation	7	2	3·5	0·408	
Handedness × method × presentation	8	2	4·0	0·467	
Error (D)	480	56	8·57		

INVESTIGATION 2

Handedness and binocular/monocular viewing

Subjects. These were 30 15-yr.-old males, comprising 15 left-handers and 15 right-handers. Handedness was determined from school records, life-history data, self-reports and the results of the Harris Test of Lateral Dominance (Harris, 1957).

Procedure. Instruction 1 was read to all subjects, and TFF thresholds obtained, using both methods A and B, under each of three viewing conditions, i.e. binocular, left eye only and right eye only. To control for serial-order effect, presentation conditions were randomized.

Statistical design. The data were organized for a three-factor analysis of variance (Winer, 1962, case 2), resulting in a 2 × 2 × 3 design. The main effects were handedness, method and presentation.

Results

A summary of the analysis of variance is given in Table 4. Method and presentation are significant effects, but handedness is not a significant variable. There are no significant interaction effects. In this analysis, method is of greater significance than in Investigation 1. Table 5 gives the mean thresholds, from which it can be seen that binocular viewing requires a shorter IFI for fusion than either left-eye or right-eye viewing for left-handers and right-handers. The IFI for right-handers tends to be longer than the IFI for left-handers under all viewing conditions and for both methods of obtaining the threshold.

Table 5. *Mean TFF thresholds for* 15 *right-handed and* 15 *left-handed subjects*

Presentation	Method	Right-handers	Left-handers	Mean
Binocular	A	40·6	45·7	43·15
	B	42·7	48·5	45·60
	Mean	41·65	47·1	44·37
Left-eye	A	45·3	47·7	46·50
	B	47·5	50·8	49·15
	Mean	46·4	49·25	47·82
Right-eye	A	42·8	48·9	45·85
	B	46·5	51·9	49·20
	Mean	44·65	50·4	47·52

Table 6. *Analysis of variance of TFF thresholds from* 15 *right-handed males and* 15 *right-handed females, using two different psychophysical methods for obtaining absolute thresholds*

Source of variance	S.S.	D.F.	M.S.	*F*	*P*
Between subjects	4897·4	29			
Sex	140·9	1	141	0·830	
Error	4756·5	28	169·87		
Within subjects	186·5	30			
Method	81·6	1	81·6	22·054	< 0·001
Sex × method	0·8	1	0·8	0·216	
Error	104·1	28	3·7		

Table 7. *Table of means showing TFF thresholds (msec.) for* 15 *right-handed boys and* 15 *right-handed girls*

	Method A	Method B
Boys	40·53	42·67
Girls	43·40	45·93

INVESTIGATION 3

Sex differences

Subjects. These were 30 high-school children, aged 15 yr., all right-handed, divided into two equal groups of males and females.

Procedure. Instruction 1 was read to all subjects, and TFF threshold obtained by both methods A and B. To control for serial-order effect, both methods A and B and the ascending and descending trial of method B were randomized within each group.

Statistical design. The data were arranged for a two-factor analysis of variance. The main effects were sex differences and method.

Results

A summary of the analysis of variance is given in Table 6. Method is again a highly significant effect, but sex is not a significant variable. The mean scores (Table 7) indicate that males tend to require a shorter IFI for fusion than do females. Although the sex difference is small and insignificant it is the same direction as that reported by Roth (1961).

INVESTIGATION 4

Threshold variability

The subject of intra-subject variability in the TFF score has not been reported by other workers. Venables (1963*a*, *b*) reported a high reliability for the TFF score from one occasion to another. Another aspect of variability, however, could reflect the degree of certainty on the part of the subject conerning the nature of the percept (i.e. one flash or two). This is of greatest importance when the IFI is close to the fusion point. The following data were obtained from the records of the 60 female subjects whose TFF scores were considered in Investigation 1. They concern only the information from the method B trials.

Table 8. *Summary of the analysis of variance for the effects of age, instruction and method on response variability (intra-series)*

(Scores refer to the number of trials taken to change from consistent fusion to consistent discrimination or vice versa.)

Source of variance	S.S.	D.F.	M.S.	*F*	*P*
Between subjects	372	59			
Age	111·5	9	12·389	2·181	< 0·05
Instruction	6·6	1	6·6	1·161	
Age × instruction	26·6	9	2·956	0·520	
Error (A)	227·3	40	5·686		
Within subjects	240·0	60			
Method	0·9	1	0·9	0·032	
Age × method	11·3	9	1·256	0·332	
Instruction × method	8·4	1	8·4	2·172	
Instructions × age × method	68·7	9	7·633	1·973	
Error (B)	154·7	40	3·868		

Table 9. *Summary of mean intra-series TFF threshold variability for* 60 *right-handed female subjects using method B (method of limits) and instructions* 1 *and* 2

(Scores refer to the number of trials taken to change from consistent fusion to consistent discrimination or vice versa.)

	Mean no. of trials between consistent responses					
	Instruction 1			Instruction 2		
Mean age (yr.)	Ascending trial	Descending trial	Mean	Ascending trial	Descending trial	Mean
5·5	0·7	1·0	0·85	0	0·7	0·35
6·4	0	2·7	1·35	2·7	1·3	2·0
7·4	2·7	0	1·35	1·3	0·7	0·75
8·4	3·3	5·3	4·3	5·3	2·3	3·8
9·4	2·7	3·3	3·0	2·7	1·3	2·2
15·0	1·7	0	0·85	0·7	1·3	1·0
20·5	4·3	2·3	3·3	1·0	2·0	1·5
29·8	1·7	2·3	2·0	0	1·7	0·85
36·7	0	2·7	1·35	3·0	2·3	2·65
46·3	0	3	1·5	2·3	0	1·15

Variability measures. These were two types as follows:

(1) *Intra-series variability.* This refers to the variability that occurs in a single ascending or

descending run. It is measured as the *number of trials* between one consistent pattern of response and the other consistent pattern.

(2) *Inter-series variability.* This refers to the difference between a subject's TFF threshold (msec.), measured on the ascending run, and that for the descending run.

Statistical design. For the intra-series variability analysis, the data were arranged in the form of a 10 × 2 × 2 design, the main effects being age, instruction and method. The inter-series variability data were arranged in a 10 × 2 design, with age and instruction as the main effects.

Results

No significant effects were determined for inter-series variability, but age has a significant effect on intra-series variability (Table 8). The means (Table 9) show that this variability tends to increase with age, i.e. older subjects are more uncertain around the fusion point. This variability does not appear to be related to the form of instruction. For the group as a whole the range was from 0 to 8 trials.

Table 10. *Summary of mean inter-series TFF threshold variability for* 60 *right-handed female subjects using method B* (*method of limits*) *and instructions* 1 *and* 2

(Scores refer to threshold differences between ascending and descending runs, measured in msec.)

Mean age (yr.)	Inter-series variability		
	Instruction 1	Instruction 2	Mean
5·5	0·7	1·0	0·85
6·4	2·0	1·0	1·5
7·4	2·0	1·7	1·85
8·4	1·3	2·7	2·0
9·4	1·7	2·3	2·0
15·0	2·3	3·0	2·65
20·5	1·7	3·3	2·5
29·8	2·3	3·3	2·8
36·7	1·7	2·7	2·2
46·3	2·3	3·0	2·65

For inter-series variability, where the score is the difference between the TFF thresholds for the ascending and descending runs, the range was from 0 to 6 msec. (Table 10). There is a suggestion of a trend whereby instruction 2 induces greater variability. Rank-order correlations between ascending and descending TFF scores were +0·96 under both instruction conditions.

DISCUSSION

Nelson & Bartley (1965), in their discussion of the theoretical interpretation of qualitative and quantitative aspects of CFF phenomena, concluded that there were many aspects of input features which manipulated threshold and which must be understood before suppositions regarding central nervous conditions can be properly dealt with. These observations apply equally well to TFF studies.

The experiments in the current series indicate that age and method are significant variables. The IFI for fusion tends to increase with age from means of about 35 msec. at age 5 to 61 msec. at age 50 yr. The pattern of increase was found to be irregular, in this respect being similar to the findings of Mesiak (1947) for CFF. Age also increases intra-subject variability in that older subjects are more uncertain around the fusion point.

In the three fusion investigations variation of 'method' resulted in significant differences in threshold. The experimental procedure controlled for a serial-order effect, and it is clear that the modified constant method consistently requires a shorter IFI for the TFF score. Hence the investigations using the modified constant method (Venables & Wing, 1962) and those using the method of limits (Roth, 1961; King, 1962) are not directly comparable.

Variations in instructions also tend to affect the TFF threshold, but the indications are that this applies only from the age of 7 yr. Under this age, variations in instructions are meaningless, as one would expect. Although sex was not found to be a significant variable, the group differences were in the direction reported by Roth (1961).

Viewing conditions introduce a further significant variable, with binocular viewing requiring shorter IFIs for fusion than monocular for both left-handed and right-handed subjects.

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ON THE RELATIVE EFFECTS OF DIFFERENT SOURCES OF VARIATION IN DICHOTIC LISTENING PERFORMANCE

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Among the many possible sources of variation in dichotic listening performance, two in particular have been accorded considerable theoretical importance. The influence of order effect has usually been regarded as an index of the functioning of some short-term storage process. The influence of ear asymmetry has commonly been taken to reflect perceptual functioning.

Most published studies have examined only one or other of these influences, so that they have even come to be viewed as alternative 'explanations' of differential accuracy in dichotic listening performance. It is contended here that these alternatives are not in fact mutually exclusive, but merely define different sources of variation in the phenomena studied. A number of different comparisons must be made before the relative importance of each of these sources of variance in dichotic listening performance can be determined.

It is now more than a decade since Broadbent (1956) began to publish a series of papers on successive responses to simultaneous stimulation. He showed, for example, that when digit-span stimuli are relayed at about 2 digits per sec. through headphones off stereophonic tape, one span to one ear and, simultaneously, a different span to the other ear, young normal subjects can reproduce the digits sequentially. It was usually found that the material delivered to one ear was first recalled, then recall of the digits delivered to the other side was next attempted: ear order tended to prevail over temporal order. Furthermore, the first set reproduced commonly contained fewer errors than the second, constituting a kind of serial-order effect. These studies have been accorded the general label of dichotic listening performance (DLP). Broadbent (1956) found similar results when different spans were presented to the eye and ear together, showing that these phenomena are not peculiar to hearing, but may be generalized to different sense modalities. This finding has been confirmed by others, including Rabbitt (1962) and Sampson (1964), for vision alone.

Since the initial observations were made, much interest has come to focus on different sources of variation of accuracy in DLP. Studies have been carried out to assess the effect of the amount of material and the rate of presentation upon subsequent report (Bryden, 1962). Others have examined the effect of the nature and meaningfulness of the stimulus material on recall (Gray & Wedderburn, 1960; Yntema & Trask, 1963; Bryden, 1964; Dodwell, 1964; Emmerich *et al.*, 1965). Those sources of variation in DLP which, however, may have the broadest theoretical implications are two that might be labelled 'order effect' and 'ear asymmetry' respectively.

At present, the attention of different investigators seems to have become focused almost exclusively on one or other of these two sources of variance, so that these have now almost achieved the status of alternative hypotheses about DLP. Thus Bryden (1967) has evaluated some models of ear asymmetry or laterality effects. One model he erroneously attributes to the present author. This, he argues, implies that laterality effects in DLP are entirely due to order effects; he then produces evidence that he believes refutes this view.

The latter model is, however, one that I would wish neither to construct nor maintain. My view is that since there are different sources of variation in accuracy of report in DLP it is desirable that attention should be paid to all the known sources of variance if the maximum amount of information is to be extracted from such investigations. To support this contention it is necessary to give a brief account of the development of the studies in this area.

ORDER EFFECT AND SHORT TERM STORAGE

Broadbent's (1957, 1958) original hypothesis suggested that recall after simultaneous presentation of different spans through different sensory channels (e.g. both ears at once) might require both a perceptual and a storage system. The perceptual system passes information successively, whereas the storage system holds any excess information that arrives while the perceptual system is already occupied in dealing with information from another channel. In DLP the digits reproduced first have been processed directly through the perceptual system, whereas the material recalled second has spent some time in storage. Material held in store is subject to trace decay and interference, so that more errors appear in the material reproduced second in sequence.

This hypothesis has been used (Inglis, 1960) to predict that senile disorder should affect the kind of short-term memory process involved in DLP. This expectation has been confirmed in studies by Inglis & Sanderson (1961), Caird & Inglis (1961) and Caird & Hannah (1964). It has consistently been found that memory-disordered patients make more errors than the non-memory disordered in the sets of digits recalled second in sequence. They behave as if their short-term memory storage system functioned very poorly, if at all.

It was further expected (Inglis, 1962*a*) that normal ageing might have a similar, if not so drastic, effect on this same process. Studies by Inglis & Caird (1963), Mackay & Inglis (1963) and Inglis & Ankus (1965) supported this expectation. Substantial confirmation of these results has also been independently secured by Broadbent & Gregory (1965), by Caird (1965) and by Craik (1965).

These findings lend additional plausibility to the original hypothesis that an important part of DLP involves a short-term memory store and that its function is reflected in order effect.

EAR ASYMMETRY AND PERCEPTUAL FUNCTIONING

In the meantime, however, other investigators had begun to formulate a hypothesis which ascribed great importance to ear asymmetry as a source of variation in DLP. This view was first put forward by Kimura (1961*a*, 1963*a*), who suggested that differential accuracy in recall is mainly due to the superior perception of auditory stimuli, under conditions of competition between the two auditory pathways, by the ear contralateral to the cerebral hemisphere dominant for speech. Since this is most often the left hemisphere, her expectation was that the material read into the right ear would tend to be more accurately reproduced than the material presented, at the same time, to the left ear under these experimental conditions.

Results which apparently support this alternative view have come from a wide range of studies. Kimura (1963*b*) has found this effect in children. A similar right-ear superiority has been reported for young adults by Bryden (1963) and by Dirks (1964). This effect has also been found in young adults tested for the recognition rather than the recall if dichotic digits by Broadbent & Gregory (1964).

In addition there is evidence that both handedness and cerebral dominance for speech influence DLP (Kimura, 1961*b*; Bryden, 1965; Satz *et al.*, 1965).

These findings lend plausibility to the hypothesis that differential accuracy in perception, due to cerebral asymmetry, accounts for much of the variance found in DLP, especially as measured by right- and left-ear recall.

It has, however, been pointed out by the present author (Inglis, 1962*b*; 1965; 1966) that any defects in DLP reported in the form of 'total recall errors', of the kind used in most of the studies cited above, cannot help us to decide about the *relative* influence of order effect and ear asymmetry. Unless both laterality *and* order of report are controlled it is quite impossible to determine what may be the most powerful influence upon recall. Errors of reproduction might be due either to a failure of part of the input to enter the system (i.e. a perceptual defect), or to a decay or distortion of input after it has entered the system (e.g. a storage defect), or both together. It has not been my purpose, as Bryden (1967) mistakenly claims, to insist that one effect is reducible to the other, but simply to point out that different kinds of analyses must be undertaken before the comparative importance of even just these two sources of variation can be estimated. It is certainly desirable to separate out what is contributed by each of these to performance in dichotic listening, since knowledge of both input and storage functions is needed for a better understanding of human behaviour.

Reconciliation

A schematic representation of two kinds of outcome in DLP, one in which the primary influence is ear asymmetry, with only slight order effect, the other in which the main source of variation is order effect, with only slight influence of ear asymmetry, is shown in Fig. 1. It can be seen that if no distinction is made between the occasions on which the right ear is used as the first channel of recall and those on which the left ear is so used, but only some kind of mean accuracy score for each ear is calculated, then a similar outcome will be found in both situations, since $E > F$ and $O > P$. In both situations, too, it is possible to claim that the right ear as the first channel has an advantage over the left ear as first channel, since $A > D$ and $K > M$: and also that the right ear as second channel has an advantage over the left ear as second channel, since $C > B$ and $N > L$. If only these analyses are made, the weight of the evidence will seem to suggest that ear asymmetry, if not the only source of variation, is by far the most powerful source. Before this conclusion can validly be reached, however, it is necessary to determine by a different set of comparisons whether the first or the second situation represented in Fig. 1 actually obtains.

As Bryden (1967) has pointed out, the evidence as a whole suggests that, when the right ear is designated as the first channel of recall, then it will indeed certainly have some advantage, thus both $A > B$ and $K > L$. The crucial comparison for evaluating the importance of order effect as compared to the influence of ear asymmetry comes from the condition in which the left ear is designated as the first channel of recall. If the outcome here is, as in the first situation in Fig. 1, $C > D$ then we would be forced to conclude that ear asymmetry is the main source of variation, with order effect having only a weak influence. If, on the other hand, as in the second situation in Fig. 1, $M > N$, we would be forced to conclude that order effect is a powerful source of variance which can indeed, under particular conditions, outweigh the influence of ear asymmetry. If the data are regarded in this light, it can be seen that we are not dealing with two incompatible hypotheses about DLP, but are engaged in the more profitable pursuit of differentially important sources of variation in DLP.

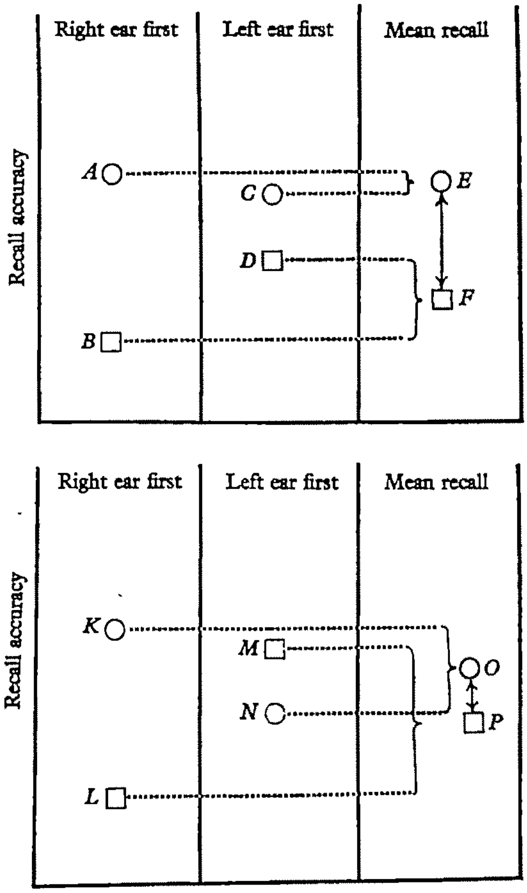

Fig. 1. Schematic representation of two kinds of possible influence of order effect and ear asymmetry on DLP. ○, right ear; □, left ear.

Very few studies have, as yet, addressed themselves directly to this latter problem. There has developed, instead, a trend for two groups devoted either to order effect or to ear asymmetry, each to analyse its findings in a way that ignores the influence of the alternative source of variation.

Investigations have been carried out by the present author and his associates that have tried to separate out the effects of age, order of recall and ear asymmetry, and these may serve as further illustration of the contentions made here.

In these studies, 240 subjects were examined by two investigators. Sykes (1967) tested 120 children between the ages of 5 and 10 yr. There were 20 children in each year group, 10 of these being boys, 10 girls; all were judged to be right-handed. Ankus (1965) tested 120 persons between 11 and 70 yr. There were 20 persons in each decade group, 10 male and 10 female. Of these 120 subjects only five were left-handed. The test digits were played over earphones at the rate of 1 digit per ⅔ sec. Various lengths of the dichotic digit span were delivered under three conditions: free recall, recall order specified before the delivery of the digits, and recall order specified after the delivery of the digits. Only the results obtained for 3 digits per ear under the latter condition will be considered here. It is evident that, when order of recall is fixed by the experimenter and such specification is made after digit delivery, then the effects of both spontaneous ear preference and differential attention are reduced, if not eliminated.

The side that had to be recalled first under each condition was indicated by means of a signal panel set in front of the subject simply bearing the printed words 'right' and 'left'; a red light was switched on above the appropriate word by the experimenter The actual side required for recall was also alternated between one half of the subjects and the other. Each subject was signalled to recall the right ear first on half the occasions and the left ear on the other occasions. The right and left series were demanded in random order. The results were scored in terms of mean accuracy over four trials for each ear where the criterion of accuracy was fixed by the demanded order of recall. It has been shown elsewhere (Inglis & Tansey, 1967) that different methods of scoring do not eliminate age differences.

It was predicted, in this case, that order effect would be an important source of variance, and that while age would not affect the material recalled first in sequence, it would affect the material recalled second in sequence, because on Broadbent's hypothesis, this material would have spent some time in a short-term memory store. It was further anticipated that there should be a curvilinear relation between age and this short-term storage function. It was expected that in the first years of life there would be a development of this storage capacity, reaching a maximum at or about the third decade, with an involution of ability appearing thereafter. It was also expected that ear asymmetry might constitute another weaker source of variance. The results obtained in the present experiment are shown in Fig. 2.

These findings show that one major source of difference is certainly order effect. At each age the material recalled first in sequence (whether delivered to the right or to the left) contains fewer errors than the material recalled second.

The age effects were estimated by trend analyses of variance (Edwards, 1960) and show a barely significant increase in children from 5 to 10 yr. for material recalled first originally read to the right ear. The left ear first shows an insignificant change. The material recalled second in sequence by these children shows a marked increase in ability with age.

For the 11–70-yr.-olds there is no significant change in the left ear first but a slight, significant decrement in recall from the right ear first. There is a sharp and significant decrement, as expected, in all the material recalled second in sequence, whether originally delivered to the right or the left ear.

The only significant ear asymmetry is shown by age-groups 6–9 for the material recalled first in sequence, the right-ear material showing fewer errors in these groups. While it is, perhaps, not too surprising that the so-called 'perceptual channel' should be the one to show the most 'perceptual' effect, the difference in this regard between the children and adults in this study would still remain to be explained. These differences are, in any case, not consistent over various lengths of span or for different conditions of recall (Inglis *et al.*, 1968).

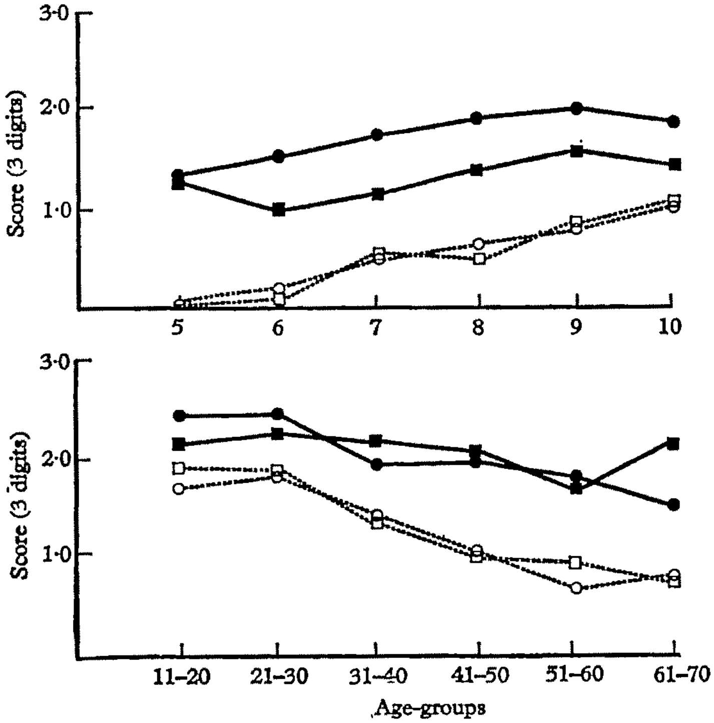

Fig. 2. Reproduction of dichotic digits with order of recall specified after digit delivery (*A* condition). Performance of groups between the ages of 5 and 70 yr. on 3 digits per half-span. *A*, condition—first recall: ●, right ear; ■, left ear: second recall: ○, right ear; □, left ear.

In terms of Fig. 1, however, it is clearly the case that $M > N$, *not* $C > D$; or, in other words, order effect, under conditions of controlled recall, can certainly outweigh the effects of ear asymmetry.

On the basis of these data it must be concluded, as Broadbent (1958) originally suggested, that order effect is a powerful source of variation in DLP, as also is age. Ear asymmetry, on the other hand, would appear to be a much weaker source of variation. This is not to suggest that the latter source of variance should be ignored, but simply to note that account should be taken of its influence relative to other factors that also influence performance.

A further confirmation of this view is also to be found in Bryden's (1967) own data in which the influences of ear asymmetry and order effect are separately illustrated. In his case, admittedly using data obtained from only 24 young subjects, the differences ascribable to order effect are, by inspection, very much larger than the differences due to ear asymmetry.

It may be reiterated, then, that it would certainly be misleading to maintain that laterality effects in dichotic listening are necessarily reducible to order-effects, or vice versa. I would only insist that adequate analyses of DLP data must certainly attend to *all* the sources of variance that seem likely to be of importance, two of which have often been confounded in previous studies. These can, however, be separately examined by the kinds of comparison outlined and exemplified above. These analyses suggest that order effect, and, by hypothesis, short-term storage, has a strong effect on accuracy of report in DLP. This means that Broadbent's original model is one that still deserves close consideration in the further exploration of DLP. The weaker effects produced by ear asymmetry are, of course, also important in their own right, perhaps as reflecting an asymmetry of cerebral organization. It attention be directed to one effect alone, at the expense of the other, there is a danger of ignoring interesting observations that may be of practical and theoretical importance for our understanding of both perception and learning.

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INFORMATION THEORY AND STIMULUS-INDEPENDENT THOUGHT

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The production of stimulus-independent thought (e.g. fantasy and imagery) was measured as a function of the rate at which information was presented to human subjects. Information in the form of simple tones was presented at rates from 0·2 to 6 bits per sec. The linear regression of reported stimulus-independent thought on information rate accounted for 83 per cent of the between-cell variance. The results support a model in which both sensory and memory events are operated on by a common central cognitive unit.

The intrusion of unsolicited thoughts, images or fantasy while engaged in reading, driving or listening to a lecture is an experience familiar to everyone—the stream of consciousness in William James's classical phrase (1890). Such thoughts may supplement the primary task of driving an automobile, or, when listening to a lecture, may altogether override one's intended goal. These illustrations emphasize the large fraction of one's cognitive activity that is independent of the concurrent external stimuli to which one is exposed. This cognitive activity corresponds, in the vernacular, to thought, imagery and fantasy, as distinct from sensations and perceptions. The distinction concerns the extent to which conscious cognitive activity 'follows', or covaries in time, with stimulus events external to the cognitive system. Perceptual events are highly correlated; fantasy may be almost independent. The contrast is sharpened by an analogous distinction in computer systems. To the extent that the human cognitive system not only receives and stores but also carries out subsequent transformations on its information input, it behaves like a computer that operates sometimes on information as it arrives in 'real time', sometimes on information previously stored in memory, or simultaneously on both.

The present experiment is one of a series concerned with the rules that determine when a cognitive system operates on sensory input—real time processing—and when it operates on events previously stored in memory. Using an information-processing approach to the problem, both sensory–perceptual and non-perceptual events are assumed to be outputs of a common limited-capacity cognitive operator. It is further assumed that a pay-off value is associated with each perceptual or non-perceptual operation which the cognitive operator might make, and that the cognitive system operates to maximize the total pay-off of these operations. If the input from combined sensory and memory input presented to the cognitive system exceeds the capacity of the cognitive operator, the total pay-off may be maximized by gating out input associated with low pay-off. Prior to a job interview, an examination or marriage, one may experience some difficulty in maintaining one's thoughts on lectures, reading and other tasks. The intruding, often repetitive thoughts may, as undeliberate planning, achieve their 'pay-off' in improved performance in subsequent overt behaviour. More typically, however, daydreaming and similar transformations on memory do not interrupt the response to sensory input, e.g. when driving a car. As highway traffic becomes heavy, one's fantasy and task-irrelevant thought is reduced. Experimental evidence for this strategy was obtained in an auditory signal-detection task, where Antrobus *et al.* (1966) found that increasing the monetary pay-off for correct detections and increasing the signal presentation rates were both associated with a decrease in stimulus-independent cognitive events. This well-defined class of cognitive events corresponds, in the vernacular, to thoughts and images that are unrelated to the task at hand, in this case, the detection task, and includes thoughts about one's family, friends and occupation.

The results of the first study confirmed the predicted effect within a limited range of information rate. Presenting information at rates of $\frac{1}{3}$ bit per sec. and 1 bit per sec., the experiment demonstrated the expected negative effect of increased task information rate on the probability of a subject's response that a stimulus-independent thought had occurred within a trial.

The present experiment is designed to test a stronger proposition concerning the relationship. If a common cognitive operator processes both sensory and memory data, then the amount of operator capacity required to generate a stimulus-independent cognitive event is equivalent to the capacity required to encode some fixed amount of information from sensory channels, where time is constant. For example, suppose that when information is transmitted at the rate of 1 bit per sec. over a large number of 15 sec. trials, a subject indicates the occurrence of some stimulus-independent thought on 60 per cent of the trials; when information is presented at 2 bits per sec., stimulus-independent thought drops to 50 per cent. Presumably, the increase in task-information rate reduces the portion of operator capacity available for processing memory channels. Suppose now that the task information rate is increased to 3 bits per sec. If stimulus-independent thought is generated in a simple sequence of elementary cognitive events, then increasing task load should delay the cognitive sequence and lower the probability of a sequence reaching the output of the operator and initiating stimulus-independent cognitive response. Given this assumption, the cognitive response should be a negative linear function of task-information rate. Three bits per sec. should yield positive responses on 40 per cent of the trials. If, however, a certain minimum channel capacity is required to generate a stimulus-independent thought, then the probability of a cognitive response may drop off precipitously once a critical task load was reached. On the other hand, some memory events may be associated with higher pay-off than others, so that each increment in task-information rate is associated with a progressively smaller decrement in the probability of a cognitive response. The experiment is designed to compare these alternative models.

METHOD

Information rate. Each subject was required to detect tones which were presented to him in a randomly ordered series at a fixed presentation rate (see Fig. 1). Three presentation rates were employed in any given trial: one presentation every 5 sec., one per sec., or one per 0·5 sec. Each trial was 15 sec. in duration and therefore consisted of 3, 15 or 30 presentations respectively. At each presentation occasion, a given tone was either present or absent. Each tone therefore constituted a binary or two-position digit (one bit of information). The probability of a selected tone occurring on a given presentation was 0·5. The order of occurrence/non-occurrence was random, with the restriction that the number of occurrences minus number of non-occurrences within a trial should not exceed 1. Since one, two or three specific tones could be programmed for a given trial, 1, 2 or 3 bits of information per presentation could be achieved. By crossing presentation rate with number of tones, information presentation rates from 0·2 bits per sec. were obtained (see Table 1). Each of the nine conditions was replicated 20 times for each subject in four sets of five trials each, making a total of 180 trials per subject. Nine different random orders of the nine combinations were employed.

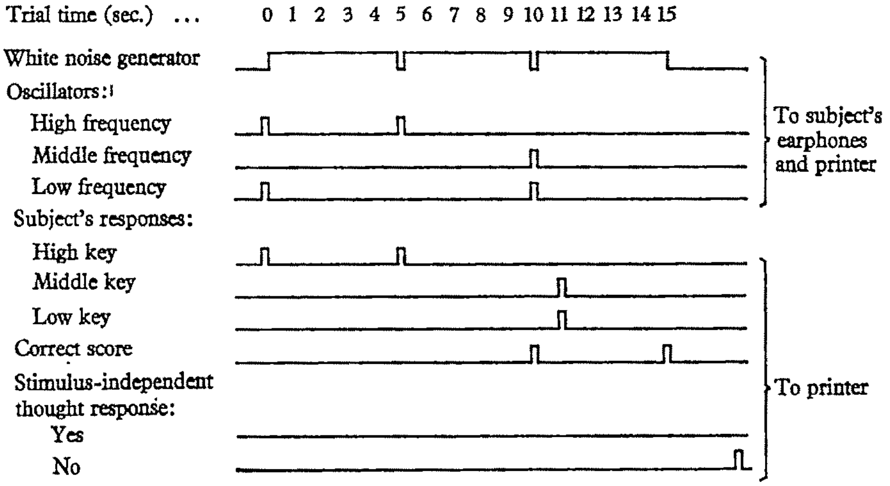

Fig. 1. Schematic diagram of 15 sec. trial; three possible tones per presentation. One presentation per 5 sec. Information rate: 0·6 bits per sec. presented; 0·4 bits per sec. encoded.

Table 1. *Information rate in bits per second under three presentation rates crossed with three tone conditions—multiplicative rule*

Number	Presentation rate		
of tones	1 per 0·5 sec.	1 per sec.	1 per 5 sec.
1	2	1	0·2
2	4	2	0·4
3	6	3	0·6

In order to score a correct detection, the subject was required to press any of three telegraph keys corresponding to the tones presented. Each presentation was 0·1 sec. in duration. A response was scored correct if it occurred before the termination of the next signal. No systematic record of response latencies was made.

Reduced non-task stimulation. In order to evaluate a simple two-channel (external stimulus *v.* memory) model, it was desirable to minimize, mask or randomize all non-task stimuli. That is, the auditory task signals should constitute the only source of non-memory information. If other classes of stimuli were available, a subject might respond to them in preference to events from memory, thereby masking the effect of task-information rate on response to memory. Accordingly, no light was available in the subject cubicle. Extraneous noise was attenuated by the 'room within a room' construction of the cubicles, and earphones which covered the entire outer ear. A moderate white masking noise in the speech range was automatically switched on between each signal presentation. Distracting proprioceptive stimuli were minimized by maintaining the physical comfort of the subject.

Stimulus-independent thought. A thought or image was defined as stimulus-independent if one or more of the referents of elements of the thought referred to a stimulus event which occurred outside in time of the immediately preceding 15 sec. trial. At least one of the referents must describe events recalled from prior to the trial, or expected or imagined to occur at some point subsequent to the trial. For example, a fantasy about his girl friend or conjecture about the purpose of the experiment was judged stimulus-independent. Concluding that he missed the last three signals would not qualify as a stimulus-independent thought. The subject was furnished with several such examples of stimulus-independent and stimulus-dependent thoughts, and, after exercising his judgement on a series of practice trials, was 'checked out' on a final set of test examples. A copy of the complete instructions may be obtained from the author.

Although the occurrence of a stimulus-independent thought may be defined with little apparent ambiguity, the distinction between one or more such thoughts is an arbitrary affair at best. It is for this reason that subjects were not asked to scale the magnitude of their stimulus-independent thought. The subject was trained to make a simple binary response at the end of each trial: 'No' if he could not recall that a stimulus-independent thought had occurred during the immediately preceding trial; 'Yes' if he could recall that at least one stimulus-independent thought had occurred within the trial. He indicated his judgement by pressing a switch in one of two possible directions. His response was manual, not verbal. He was not asked to report his thoughts. It was possible for the subject to forget the specific content of a thought but to recall that a stimulus-independent thought had occurred.

Apparatus. Each trial was initiated by a 'Yes' or 'No' response from the subject; stimuli and trial boundaries were programmed on paper tape. Responses were scored electronically and printed on paper tape at the end of each trial.

Subjects. Subjects were 50 undergraduate students solicited from the College Placement Office. They were offered $2·00 per house bonus, adjusted according to their performance. The bonus or pay-off clearly established signal detection as the primary task and reporting of stimulus-independent thought as secondary. There was no pay-off for reporting more or less stimulus-independent thought.

Procedure. After the subject was comfortably seated in the sound-attenuated booth, he was given verbal instructions by the experimenter. The subject was trained to detect first one, then two, and finally the three possible tones in all their combinations and at the three different presentation rates. He was told to press the telegraph key or keys which corresponded to the presented tones. During the initial portion of the practice session, the correct keys were identified by pilot lights which appeared simultaneously with the tones. The practice period lasted from 20–30 min., until the subject achieved a detection criterion of 2 bits per sec. without the aid of the pilot lights. Following a 5 min. break outside of the booth, the subject was trained to judge the occurrence of stimulus-independent and stimulus-dependent thought, and commenced the experimental trials. The entire experiment, including practice and rest periods every ½ hr., lasted 2½ hr.

RESULTS

The proportion of stimulus-independent thought responses in each condition is plotted in Fig. 2 as function of the rate at which information was presented. The total between-cells sum of squares = 1330, of which 1106 can be accounted for by the linear regression of reported stimulus-independent thought on the nine information rates (see Hays, 1963, chap. 12–16; Scheffé, 1959). The linear information rule therefore accounts for 83 per cent of the between-cell variance, of the variation among the nine conditions, and is highly significant ($F = 82{\cdot}99$; 1, 392 d.f.; $P < 0{\cdot}01$). The data strongly confirm the hypothesis that the separate effects of stimulus density and presentation rate on stimulus-independent thought are largely accounted for by an information rule: Stimulus-independent thought decreases as a linear function of the information rate of presented auditory signals.

The remaining 17 per cent of the between-cell variation consists of residual deviations of the nine cell means about the linear regression line predicted by information theory. Four tests were performed on the residual variation. Are there linear and quadratic effects due to differences in number of tones, and differences in presentation rate, after the effect of information rate has been subtracted? First, the linear and quadratic effects for tone and rate were computed. Then the linear information component was removed from each of the four effects and F tests on 1 and 392 d.f. were computed on each. A small linear tone effect was identified ($F = 5\cdot6$; $P < 0\cdot02$), and also a small quadratic rate effect ($F = 3\cdot82$; $P < 0\cdot06$). The quadratic effect for the rate and the linear effect for number of tones were not significant. The linear effect for tones is apparent in Fig. 2, where one tone consistently rides above the regression line. Relative to its information rate, one tone has less of a negative effect on stimulus-independent thought than two or three tones. The possible quadratic effect for presentation rate suggests that stimulus-independent thought is a negative decreasing function of rate, so that with additional increases in presentation rate smaller decrements in stimulus-independent thought may be achieved. In summary, the statistical analysis consists of five independent tests, each on 1 and 392 d.f. The linear information hypothesis accounts for almost all of the between-condition variance.

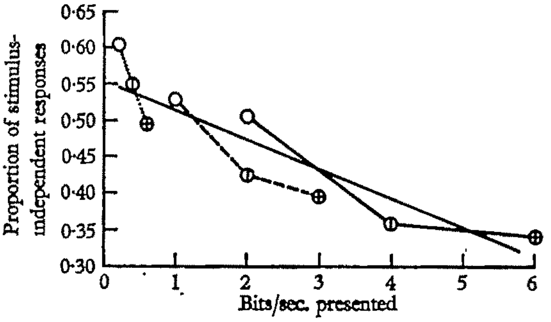

Fig. 2. Relative frequency of stimulus-independent thought reports as a function of the rate of presenting information. . . . . ., 1 per 5 sec.; - - - - -, 1 per sec.; ——, 1 per 0·5 sec.; ○, one tone; ⦶, two tones; ⊕, three tones.

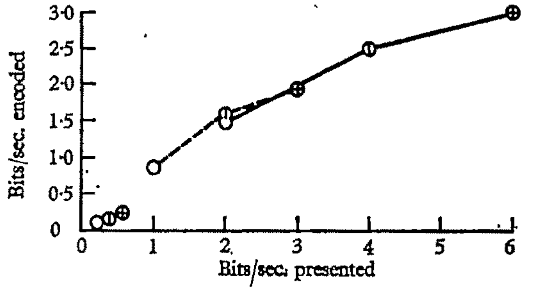

Fig. 3

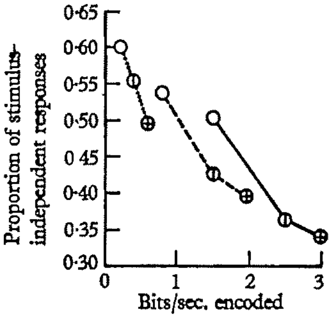

Fig. 4

Fig. 3. Rate of encoding information plotted as a function of rate of presenting information. Symbols as in Fig. 2.

Fig. 4. Relative frequency of stimulus-independent thought reports as a function of the rate of encoding information. Symbols as in Fig. 2.

Information *encoded* is a negatively increasing function of the rate of information presented (Fig. 3). Consequently, a plot of stimulus-independent thought against rate of encoding information is presented (Fig. 4). Although the presentation in Fig. 4 has not been analysed statistically, the truncation of the information axis appears to have the effect of reducing the proportion of between-cell variance accounted for by an information (encoded) rule.

DISCUSSION

At the most general level, this experiment demonstrated that precise measurements can be made on reports of essentially private events—one's thoughts and images. The reports are sufficiently lawful to permit the evaluation of different models of mentation. The large proportion of between-cell variance (83 per cent) accounted for by a simple row × column multiplication rule on 1 d.f. is a considerable accomplishment for the information model.

Table 2. *Additive rule for combining row and column effects*

Number of tones	Presentation rate 2 per sec.	 1 per sec.	 1 per 5 sec.
1	3	2	1·2
2	4	3	2·2
3	5	4	3·2

Of the many possible alternative models, the most obvious is a simple additive model (see Table 2). In the multiplicative model three tones have, by definition, three times the effect of one tone at each presentation rate. The absolute difference between three and one tones, however, greatly diminishes as the presentation rate decreases. In the *additive* model the absolute difference between three and one possible tones remains constant across all presentation rates. Fig. 2 shows that the information model provides the poorest fit at the slow rate. The greater separation of the three tones on the abscissa based on the additive rule would clearly improve the fit at the slow end of the scale.

Comparison of the variance accounted for by the two models shows that the linear components for tones and rate sum to 1194, or 90 per cent of the between-cell variance, 7 per cent more than that predicted by the multiplicative information rule.

Balanced against this 7 per cent advantage is the *post hoc* character of the additive combination. There exists no fundamental theory which predicts the weights by which rate and tone number might summate. The specific weights used in the additive model (Table 2) are therefore somewhat arbitrary. In the absence of such a rule, the additive effects of tone and rate must be referred to 2 d.f. The information rule is based only on 1 d.f., and is accordingly more parsimonious.

Formal limitations of the additive model also indicate the superiority of the information model. The additive model predicts a tone effect where rate is reduced to 0 per sec., and conversely, a rate effect if 0 tones are presented. In both instances the information rate (multiplicative model) properly predicts zero effect.

Empirical scaling procedures (Luce & Tukey, 1964) might be used to estimate the optimal weights to use for combining rate and tone effects. In view of the very high proportion of between-cell variance already accounted for by simpler alternatives, the advantage of such a *post hoc* procedure, which would require additional degrees of freedom, would be minimal.

In conclusion, the unequivocal evidence for the linear regression of stimulus-independent thought on task-information rate supports the original proposition that, as available operator capacity decreases, there is a corresponding decrement in the rate of carrying out the elementary cognitive operations or transformation which precede a subject's awareness of a thought or image. As described in the introduction, the cognitive model also assumes that memory and sensory inputs to the operator are co-ordinated so as to maximize pay-off within the constraints of the operator's capacity. The production of stimulus-independent thought may be associated with a higher pay-off than operating on detection-task signals. The model assumes that the potential pay-off for operating on memory events varies from one event to the next. A given memory event may therefore have greater or lesser significance, or value, than a task signal. When the capacity of the operator has been attained, the ideal strategy should select memory events in preference to task signals only when the value, or pay-off, for a specific memory event exceeds that for the task event. This is consistent with the observation that, as the detection error rate reaches 50 per cent stimulus-independent thought continues to be reported on 34 per cent of the trials (Figs. 3–4). According to the model, the higher error rate should be associated with the elimination of stimulus-independent thought only if pay-off for signal detection was greater than for response to every memory event which reached the gate. In less extreme conditions, where some memory events are associated with higher pay-off values than task events, stimulus-independent thought should reach some lower limit. This lower limit would be reached at the point where the gain in pay-off for increasing the rate of detecting signals equals the consequent loss in pay-off for gating out high pay-off memory events. At such a point the regression line in Figs. 2–4 should have zero slope. Figs. 2–4 identify small quadratic effects for both presentation rate and number of tones in the predicted direction or decreased slope at higher information rates. The quadratic effects account for only 4 per cent and 3 per cent, respectively of the between-cell variance, and do not reach significance at the 0·05 level. Nevertheless, the trends suggest that larger quadratic effects might appear at information rates higher than those employed in the present study.

Alternative explanations for the concurrence of stimulus-independent thought and high detection error rates require modification or elaboration of the model. The author has proposed that the operator is capable of *parallel* operations on several, in this experiment, two channels, simultaneously.

The portion of the operator's capacity required for processing task information is continuously estimated and projected into future time. The balance of this projection is available to input from memory. The obtained association in time of a high detection error rate and stimulus-independent thought is perhaps more consistent with *sequential* processing (Broadbent, 1958, 1962; Reitman, 1965; Simon, 1967), where the operator switches back and forth between sensory–task and memory channels. In sequential processing, the rate of encoding sensory information is limited by the processing rate and the short-term memory capacity of the operator—the length of time the operator can hold the auditory signals in short-term storage prior to encoding. The rate of operating on events in long-term memory would be limited by the remaining free time available when the operator is not encoding sensory or short-term memory events. The utility of a sequential processing model may be tested in a self-paced

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A FURTHER STUDY OF THE INFERENTIAL BASIS OF CONCEPT ATTAINMENT

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Time scores were obtained from five groups of subjects, ranging in average age from 8 yr. to 62 yr., on six types of concept-attainment problem presented in familiar and unfamiliar symbols. The results for each group were subjected to analysis of variance. The effects of size of problem and type of symbol employed were highly significant irrespective of age. The effect of type of problem (presenting positive or mixed instances) was significant in all but the 62 yr. group, and the interaction between type and size of problem in all but the 38 and 62 yr. groups. The interaction between type of problem and type of symbol employed was, however, significant only in the 38 and the 62 yr. groups. An interpretation is suggested for these differences. In the three adult groups, problem-solving proficiency was shown to correlate with short-term memory capacity for the familiar and unfamiliar symbols employed.

A previous paper (Wetherick, 1966) reported three studies of the inferential process underlying concept attainment, that is, of the process whereby a subject concludes, from an examination of positive or mixed positive and negative instances of a concept, that a certain subset of attributes defines positive instances. In these studies, only error scores were obtained since it was essential to show that errors were not the result of time pressure. The main purpose of this further study was to examine the time required for solution. In addition, the writer became interested in the effects of presenting the problems in symbols unfamiliar to the subject (Hebrew letters) in place of the familiar, highly overlearned symbols (English letters) that had previously been employed. Solving one of these problems, using familiar and unfamiliar symbols, may be regarded as analogous to solving a real inductive problem involving attributes which are familiar and nameable as opposed to being unfamiliar and as yet unnamed. It has been suggested that the progressive intellectual decline associated with age is relatively more important where the task is unfamiliar to the subject (typically, scores on the progressive matrices decline with age whereas scores on vocabulary tests do not); and so, it would follow that the time required to solve one of our problems in unfamiliar symbols should increase more sharply with age than the time required to solve the same problem in familiar symbols. We may also predict that problems presenting more instances will take longer to solve. A further prediction might be that problems presenting mixed positive and negative instances will take longer to solve than problems presenting positive instances only; because problems of the latter kind are solved when a common subset of attributes has been found, whereas in mixed problems, common subsets of attributes have to be found and discarded, the solution being the subset remaining in the positive instance.

METHOD

Procedure. A set of 24 problems was presented to the subject, once in familiar symbols (English letters) and once in unfamiliar symbols (Hebrew letters). Alternate subjects did the two versions in the order English/Hebrew and Hebrew/English. In each case, the subject was presented with a key card, bearing the 20 (familiar or unfamiliar) symbols, on which each symbol was associated with a number from 1 to 20. The problems were presented individually and the subject's task was to select what he thought was the correct symbol and, by reference to the key card, report verbally the number associated with it. A stopwatch was started as the problem was presented and stopped when the correct response was given; if the subject gave a wrong response the experimenter said 'wrong' and the subject tried again. Too many wrong responses might have distorted the average time scores but the fact that a simple type of problem was employed and that the problems were presented in condition B (in the terminology of the previous paper) served to minimize the incidence of error. Each problem presented either two, three or four symbol-groups, each group consisting of four symbols: the answer was always a single symbol. The instances presented were either all positive or mixed (one positive and one, two or three negative). Four examples were presented of each of the six types of problem and the score obtained was average time (in sec.) required to solve each type. Examples are given below:

Ex. A In BKMS Not in BHMS (Answer 'K')
Ex. B In CLMR, DLMV, CKMV (Answer 'M')

Subjects. Fifty subjects were employed (23 male and 27 female). All were of high educational attainment or potential. The adults were either university graduates, undergraduates, or similar: the children were selected as likely to proceed to some form of higher education. The children's ages varied from the mean for their group (8·3 yr. and 13·5 yr.) by not more than a few months. The adult groups ranged in age from 18 to 24 yr. (mean 20·9 yr.), from 29 to 45 yr. (mean 38·2 yr.) and from 51 to 69 yr. (mean 61·8 yr.).

RESULTS

No effect was observed of sex or of order of attempting the two forms of the task. Errors were infrequent and were spread reasonably evenly over the types of problem so that there was no differential effect on average time required for each type. The skewing effect of occasional very long times to solution was largely eliminated by tabulating averages of four times for each type of problem. These averages are shown in Table 1.

The results for each group of subjects were subjected to four-way analysis of variance (McNemar, 1962, case XII) to distinguish the effects of size of problem (G), type of problem (T) and type of symbol employed (L). In such analyses, each of the three main factors and their interactions are evaluated against the interaction of next higher order involving subjects (subjects constitute the fourth factor in the analysis but the interactions involving them are not testable for significance). In this experiment, comparisons between age-groups can be made on the basis of variance ratios, and Table 2 shows the ratios obtained in each of the five groups.

From Table 2 it appears that, as hypothesized, factor G is highly significant in all groups. As problems increase in size from two to three to four symbol-groups, the time required for their solution also increases. The actual times are shown in Table 1. Note that in problems presenting mixed positive and negative instances there is a regular increase in time required as problems increase in size, whereas this is not so in problems presenting positive instances only. Here, there is an increase as one moves from two-group to three-group problems, but little difference between three- and four-group problems. This results in an interaction (G × T) between size and type of problem which is significant in the three younger groups. Moreover, in some cases, four-group problems were solved more quickly than three-group. This opened the possibility of a three-way interaction with type of symbol employed (G × T × L) and this interaction was significant in the 20·9 age-group. In that group, four-group

Table 1. *Average times to solution (sec.) of five groups of subjects for six types of problem presented in familiar and unfamiliar symbols*

(n = 10 in each group.)

	Age-group (yr.)				
	8·3	13·5	20·9	38·2	61·8
Two symbol-groups					
Positive					
Familiar	9·58	6·48	4·99	6·33	7·82
Unfamiliar	11·86	8·53	8·45	10·92	14·74
Mixed					
Familiar	14·19	7·25	5·96	8·15	9·41
Unfamiliar	17·08	8·92	8·46	12·23	14·20
Three symbol-groups					
Positive					
Familiar	13·99	8·87	6·29	8·63	9·73
Unfamiliar	21·24	11·39	10·32	15·18	25·03
Mixed					
Familiar	15·52	9·02	7·24	9·41	10·87
Unfamiliar	23·63	12·50	9·43	17·54	21·79
Four symbol-groups					
Positive					
Familiar	14·23	8·06	7·16	8·78	11·92
Unfamiliar	21·89	11·11	9·82	16·21	24·52
Mixed					
Familiar	19·42	11·51	9·26	12·45	13·11
Unfamiliar	24·87	16·47	13·85	19·52	28·13

Table 2. *Variance ratios obtained in five groups of subjects for three main factors and the interactions between them*

(n = 10 in each group.)

	Age-group (yr.)				
	8·3	13·5	20·9	38·2	61·8
G	17·2***	51·7***	25·6***	29·3***	23·7***
T	5·9*	13·4**	8·7*	9·5*	0·94
L	28·2***	27·6***	36·8***	36·1***	35·0***
G × T	6·1**	8·8**	4·0*	1·6	1·2
G × L	0·50	1·7	0·09	5·8*	5·5*
T × L	0·17	0·08	0·29	0·00	0·63
G × T × L	0·30	0·28	6·8**	0·01	0·76
Total variance	29·604	14·049	14·824	34·298	67·205
Percentage of total variance attributable to above factors and interactions	80·6	87·6	90·6	89·2	91·7

* $P < 0·05$; **$P < 0·01$; *** $P < 0·001$.

positive problems took longer to solve than three-group if they were presented in familiar symbols but not so long if they were presented in unfamiliar symbols.

Factor T was significant in all except the 61·8 age-group. Problems presenting mixed positive and negative instances took longer to solve than problems presenting positive instances only. The finding for the four younger groups accords with our hypothesis, and the question arises why this factor is not significant in the 61·8 group. This question will be considered below in the discussion.

Factor L was highly significant in all groups. Problems presented in unfamiliar symbols took longer to solve than the same problems presented in familiar symbols. This is consistent with our hypothesis but we also hypothesized that the difference would be relatively greater in older subjects. This part of the hypothesis is supported by the fact that, as appears from Table 1, the percentage increase in time required for problems presented in unfamiliar symbols is an increasing function of age in the three adult groups. Overall, the 20·9 group took 47 per cent longer, the 38·2 group 70 per cent longer and the 61·8 group, 104 per cent longer. The presence in the two oldest groups of a significant size by type of symbol interaction (G × L) lends further support to the hypothesis. In children and young adults the extra time required is a more or less constant proportion of the time required to solve the problem in familiar symbols; in the middle-aged and elderly it is a proportion increasing with the size of the problem.

The T × L interaction does not approach significance in any group. The performance of the operations appropriate to positive or mixed instances of a concept seems to be independent of the relative familiarity/unfamiliarity of the attributes exemplified by the instances.

The subjects who took part in this experiment were given a further test designed to assess the capacity of their short-term memory store for the familiar and unfamiliar symbols employed (Wetherick, 1968). In the adult groups ($n = 30$), short-term capacity, in terms of number of symbols remembered, was found to correlate with average time to solution in the problem-solving task. Tau equalled $-0{\cdot}31$ ($P < 0{\cdot}01$) for familiar symbols, and $-0{\cdot}34$ ($P < 0{\cdot}01$) for unfamiliar symbols. However, both short-term capacity and average time to solution correlated significantly with age and, when age was partialled out, the above correlations reduced to $-0{\cdot}25$ and $-0{\cdot}27$ respectively. These partial correlations are unfortunately not testable for significance.

DISCUSSION

Two of the three main factors are unequivocally significant in all groups, i.e. size of problem and type of symbol employed. It also seems clear that unfamiliar symbols are relatively more difficult to handle for older subjects. The important point here is that the problems were identical except as regards the relative familiarity/unfamiliarity of the symbols employed. Table 1 shows that, for both familiar and unfamiliar symbols, average time to solution decreases from childhood (8·3 yr.) to early maturity (20·9 yr.) For familiar symbols there is a slight increase in time required by the older groups, but the oldest subjects are still faster than 8-yr.-olds. For unfamiliar symbols, there is a much larger increase in time required, and the oldest subjects are somewhat slower than 8-yr.-olds. In this simple type of problem, age does not markedly affect ability to perform the required mental operations so long as the attributes involved are familiar. However, as was shown in the previous paper, with more difficult problems older subjects are increasingly liable to error even though the attributes are familiar.

The reason why the third main factor (type of problem) failed to reach significance in the 61·8 group is unclear. This was the age-group which, in the previous study, was particularly liable to error arising from a failure to distinguish between the procedure appropriate to positive instances and that appropriate to mixed instances, that is, between types of problem. We have to reconcile the fact that subjects of this age confuse the procedures and, consequently, make errors when attempting to solve difficult problems although, when attempting easier problems under conditions that minimize error, they take no longer to deal with mixed instances than with positive instances. The interpretation to be put upon the significant interactions obtained is also unclear. Subjects' introspections suggest the existence of a break between problems presenting two positive instances and all other types of problem whatsoever. The former can be solved by a process described by one subject as 'mental superimposition' but, as soon as more than two instances are presented, or any of the instances is negative, an iterative procedure has to be adopted in which one positive instance is taken as a base and the relevant subsets of attributes it exemplifies are taken one at a time to see whether they are present in the remaining instances (positive problems) or absent (mixed problems). A subset which meets this requirement will constitute the solution to the problem. Since there will always be a number of subsets (four in the present experiment) which might constitute the solution, it is more likely than not that the subject will consider first one or more subsets that do not constitute the solution. In positive problems, he will have to carry a subset in his head and drop it from consideration if, and only if, he sees an instance that does not exemplify the subset. Further experiments are to be undertaken to determine how subjects allocate their time to different stages of the solution procedure, and whether there is a difference between reaction time to what a stimulus is and reaction time to what it is not. It is hoped that these experiments will throw further light on the problem.

Welford (1958) and Hunter (1964), among others, have suggested that the short-term memory may be involved in higher mental processes. This suggestion receives support from the observed correlation between short-term memory capacity and inductive problem-solving proficiency. The correlation is not high, but it is not suggested that short-term capacity is the only factor involved—merely that it may be one of them.

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THE EFFECTS OF ANXIETY ON THE RELATION BETWEEN REACTION TIME AND STIMULUS LIGHT INTENSITY

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A number of investigators have found that, under normal conditions, RTs grow shorter with an increase in stimulus intensity. Other investigators have found a flattening of the curve relating RTs to stimulus light intensity and in some instances a paradoxical increase in RTs at high intensities for schizophrenics, subjects with independently measured 'weak' nervous systems and subjects administered caffeine. The latter findings have been interpreted in terms of the development of transmarginal inhibition consequent upon high levels of excitation having been reached. The findings of the study reported here suggest that these paradoxical relations between RTs and stimulus intensity do not occur in subjects classified as anxious on the basis of the Taylor Manifest Anxiety Scale. They suggest that they do occur when subjects are threatened with shock for poor performance. The effect, however, appears to be a transitory one, disappearing with repeated testing.

Experiments with normal subjects have generally demonstrated a decrease in RT with an increase in stimulus intensity (Teichner, 1954). Venables & Tizard (1956*a*, *b*) found that the RTs of non-paranoid schizophrenics to the brighter light stimuli were longer than those to weaker stimuli. They interpreted this paradoxical effect of an increase in RT with an increase in stimulus intensity in terms of Pavlov's theory of transmarginal inhibition.

Gray (1964) has given a detailed account of the work of V. D. Nebylitsyn in which it was found that (1) when strength of the nervous system is determined by the method of extinction with reinforcement of the photochemical conditioned reflex (PCR), for subjects classified as having strong nervous systems, the curve describing the relation between RT and stimulus light intensity is steeper than the curve for the weak group—the difference in slope is due primarily to the longer RTs at low intensities for the strong group; (2) caffeine reduces RTs at lower levels of stimulus intensity, this effect being more marked for strong nervous systems than for weak ones. The effects described in (1) and (2) were statistically significant for auditory stimuli but not for visual stimuli.

Gray has also reviewed the work of A. N. Vasilev on RTs to auditory stimuli which demonstrated 'that reaction time to the offset of stimulation ("RT/off") is faster at low intensities of stimulation, but, with increasing stimulus intensity, the advantage in speed of response passes to reaction time to the onset of stimulation ("RT/on")' (Gray, 1964, p. 242). Vasilev also obtained evidence to suggest that the position on a stimulus intensity scale of the point of transition from faster RT/off to faster RT/on is at lower intensities after administration of caffeine. Vasilev suggested that the superiority of RT/off at low intensities is due to the accumulation of excitation during the period the stimulus is on. The point of transition to the superiority of RT/on reflects the development of transmarginal inhibition.

Reference to the Soviet data in an attempt to support Venables & Tizard's interpretation of their own data would not be completely justified, since the experiments of Venables & Tizard involved *visual* RTs and the significant results of the

Soviet studies involved auditory RTs. In a later experiment Venables & Tizard (1958) failed to find in schizophrenic subjects an increase in RT with an increase in *auditory* stimulus intensity.

Costello (1963) failed to find any significant effects of a stimulant and two kinds of depressant drugs on the relationship between RT and stimulus light intensity.

Venables & Tizard (1956*b*) noted that the paradoxical effect of an increase in RT with an increase in stimulus intensity beyond an optimal point occurred only on initial testing. It did not occur on a second occasion of testing. They suggested that on the first occasion, in an unfamiliar situation, subjects were apprehensive and in a state of considerable emotional excitation. The excitation resulting from the stimulus lights was superimposed on the emotional excitation, became ultramarginal in strength and resulted in the paradoxical increase in RT.

The experiment reported here was designed to investigate the effects of emotional excitation on the relation between RT and stimulus light intensity. The assumption underlying the experiment is that if emotionality of a situational nature increases the probability of occurrence of paradoxical increases in RT, as Venables & Tizard suggest, then one would expect subjects with a high predisposition to develop anxiety to show a greater degree of paradoxical increase in RT than subjects low on this trait. This is an assumption that would appear to be logically defensible, though it conflicts perhaps with Gray's recent suggestion (Gray, 1967) that the extraversion–introversion personality trait is more likely than anxiety to be related to Pavlov's distinction between strong and weak nervous systems. Threat of shock was also used in the experiment in an attempt to increase the degree of situationally provoked emotionality.

METHOD

Subjects. The subjects were 30 high-anxious (HA) males and 30 low-anxious (LA) males selected on the basis of their scores on the Taylor Manifest Anxiety Scale (Taylor, 1953) from a total sample of 200 first-year male university students. The distribution of scores for the total sample ranged from 2 to 31, with a mean of 14. The HA subjects had scores of 24 and above and the LA subjects had scores of 7 and below.

Apparatus. The stimulus was an aperture 0·75 in, in diameter, illuminated from the rear by a 150 W bulb, which was fed from a stabilized voltage source. The light source was set in a 2 × 2 ft. matt black panel placed at an angle to the vertical so as to be at right angles to the subject's line of sight when seated at a distance of 2 ft. from the panel. Variations in stimulus intensity were measured by means of a photometer. The values used in the study were: intensity A, 16 ft.-candles; B, 450 ft.-candles; C, 1500 ft.-candles.

Procedure. Each group of 30 subjects was split randomly into a 'shock' and a 'no-shock' group. The members of the subgroups of 15 subjects each were randomly assigned to three different orders of the three stimulus intensities: order I, ACB; II, CBA; III, BAC.

The subjects in the 'shock' groups were told just prior to the experiment that if they did not respond quickly to the onset of the stimulus light, they would receive an electric shock through the key. No shocks were in fact used in the experiment.

Twenty RTs were measured for each stimulus intensity, though only responses 6–15 were used in the analysis to avoid warm-up and end-spurt effects.

A forewarning buzzer signal preceded by 3·0 sec. the onset of the stimulus light. The subject reacted to the stimulus light by lifting his finger. The light then went out and was prevented by a relay mechanism from immediately reappearing when the subject's finger was replaced on the key to await the next presentation of the stimulus light. If the subject failed to respond, the light remained on for 3·0 sec. In no instance during the experiment did a subject fail to respond. The cycle of buzzer and light was repeated once every 8 sec. Between each group of 20 responses, a 3 min. rest period was allowed.

Measurement of each response was made by means of a tape-recorder linked to a Lafayette stop-clock no. 20225A, accurate to the nearest $\frac{1}{100}$ sec. This method of recording RTs has been described in detail elsewhere (Costello & Stephen, 1963).

RESULTS

Analysis of variance of the data indicated that the main effect of anxiety was significant ($F = 4{\cdot}56$; 1, 48 d.f.; $P < 0{\cdot}05$) and the main effect of stimulus intensity was significant ($F = 15{\cdot}57$; 2, 96 d.f.; $P < 0{\cdot}001$). The mean RTs for the HA and LA groups were 0·33 and 0·30 sec. respectively. (The figures have been rounded off to the second decimal place to reflect the level of accuracy of the stop-clock used in the experiment.) The mean RTs for the three levels of stimulus intensity were: A, 0·33 sec.; B, 0·31 sec.; C, 0·30 sec.

The interaction of columns (the first, second or third column of 20 stimulus lights, irrespective of intensity level) with intensity, anxiety and shock were significant. The F ratios for these interactions were as follows. Columns × intensity: $F = 11{\cdot}91$; 4, 96 d.f.; $P < 0{\cdot}001$; columns × shock: $F = 8{\cdot}75$; 2, 96 d.f.; $P < 0{\cdot}001$; columns × anxiety: $F = 3{\cdot}09$; 2, 96 d.f.; $P < 0{\cdot}05$. The following second-order interaction was also significant. Columns × intensity × shock: $F = 33{\cdot}48$; 4, 96 d.f.; $P < 0{\cdot}001$.

Table 1. *Mean RTs to lights of three intensities under shock and no-shock conditions*

($n = 10$ in each cell; RTs in $\frac{1}{100}$ sec.; decimal points removed.)

	Stimulus intensity		
	A (16 ft.-candles)	B (450 ft.-candles)	C (1500 ft.-candles)
Shock	31	31	33
No shock	37	31	30

Because analysis of all the data clearly indicated that the columns effect had contributed to a considerable amount of the variance, it was decided to analyse the data for the first block of 20 stimulus lights only. This amounted to analysis of data obtained in a 2 × 2 × 3 factorial design—two levels of anxiety, two levels of shock, three levels of stimulus intensity. The main effect of anxiety was significant ($F = 6{\cdot}34$; 1, 48 d.f.; $P < 0{\cdot}05$). The means for the two anxiety groups were: LA, 0·30 sec.; HA, 0·34 sec.

The only other significant effect was the interaction between level of shock and stimulus intensity ($F = 3{\cdot}35$; 2, 48 d.f.; $P < 0{\cdot}05$). The means for the three intensity levels under shock and no-shock conditions are presented in Table 1.

A trend analysis of the linear and quadratic components of regression was done. Only the linear components were significant. The linear component of the shock × stimulus intensity interaction was significant ($F = 6{\cdot}574$; 1, 48 d.f.; $P < 0{\cdot}05$). In addition, a separate trend analysis for the data under the two conditions indicated that only the trend under the no-shock condition was significant ($F = 10{\cdot}420$; 1, 48 d.f.; $P < 0{\cdot}01$). It can be seen from Table 1 that under the no-shock condition RT decreases with increasing stimulus intensity.

DISCUSSION

An attempt was made to manipulate the level of the subjects' emotional excitement by two means—by selecting high and low scorers on the Taylor MAS and by giving half of the subjects a threat of shock for poor performance.

The analysis of the data indicates that the high-anxious subjects had slower RTs than did the low-anxious subjects. Though not in line with Spence's theory (Spence, 1958), the findings are similar to those obtained by Kamin & Clark (1957). The latter investigators also found a significant interaction between anxiety level and threat of shock *v.* no threat of shock. This interaction was not significant in the present experiment. This is a surprising result, but then the relation between individual differences in anxiety and response to stress is a complex one, as Kamin (1957) pointed out.

There was a significant interaction between anxiety level and column of stimulus lights. The RTs of the LA group do not change over the three columns of 20 stimulus lights. The RTs of the HA group drop from the first to the second column with no change from the second to the third column. This suggests that the high anxiety of the HA group interferes with performance at the beginning of the experimental session and that this is followed by an adaptation to the experimental situation, with a consequent lowering of anxiety.

When all the data of the experiment were analysed in one analysis of variance the main effect of stimulus intensity was significant and the means indicate that with an increase in the intensity of the stimulus light the RTs grow shorter. This is, of course, the relation generally found with normal subjects. In this analysis no significant interactions of intensity with either anxiety level or shock condition were found. It appears that this may be due to the overriding contribution of columns of stimulus lights to the variance. When the data for the first column of 20 lights were analysed separately, a significant interaction between shock condition and stimulus intensity was found. Table 1 indicates that under the no-shock condition the relation between RT and stimulus light intensity is the normally expected one of shorter RTs with higher intensities. The subjects under threat of shock show much faster RTs at the lowest level of illumination than do those under no-shock conditions. These fast RTs are maintained at the medium level of illumination, but increase at the highest level of illumination. When the influence of repeated testing is taken out then, the paradoxical increase of RTs under condition of emotional excitement with increasing stimulus intensity was found. Though this trend was not found to be statistically significant, the finding is generally in line with the findings of Venables & Tizard (1956*a*, *b*).

No attempt will be made to interpret the various interactions with the column effect of the other variables. The overall impression gained is that the effects of the other variables on RTs are transitory ones, not only those producing paradoxical effects in RTs which Venables & Tizard (1956*b*) have already noted, but, more importantly perhaps, even the classical relation between RTs and stimulus intensity appears to lose some of its regularity after repeated testing. More definite data in relation to this could be obtained only in an experiment where sequence and columns effects are not confounded, as they are to some degree in the present experiment.

The transitory nature of the effects on RT of the interaction between threat of shock *v.* no threat of shock and stimulus intensity does not appear to fit well the thinking of Soviet investigators. At least they do not seem to have suggested that such effects may be transitory. Though they have demonstrated that situational factors (such as drugs administered) modify any such effects relating RTs to stimulus intensity, their use of the results of such experiments in relation to their typological endeavours suggest that they consider such effects to be something more than transitory ones.

The greater importance of situational factors than individual differences in relation to the stimulus-intensity parameter is suggested in this experiment by the significant interaction of the shock condition and stimulus intensity when the first column of data was analysed and the absence of a significant interaction between anxiety level, as measured by the MAS, and stimulus intensity. On the other hand, Castaneda (1956) did obtain evidence of a significant interaction between anxiety, as measured by the Taylor MAS, and stimulus intensity. But his results were contrary to what one would predict on the explanation of paradoxical effects suggested by previous investigators and adopted in this study. Castaneda's significant interaction was based on a tendency for the speed of reaction of the HA group in comparison to the LA group to be slower at the weak intensity but faster at the strong intensity. It is noteworthy that Castaneda used an auditory stimulus. It would seem clear that systematic investigation of sense modality will be required as part of any further investigations in this area.

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A COMPARISON OF STEREOGNOSTIC RECOGNITION IN NORMAL CHILDREN AND SEVERELY SUBNORMAL ADULTS

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Recently some doubt has been cast on the generality of Hermelin & O'Connor's (1961) finding that imbecile children perform better on a tactual exploration–tactual recognition task than do normals of equivalent mental age. The present investigation is in part a replication of the original study, using normal children and SSN adults matched for mental age. The same subjects are used to investigate differences in stereognostic recognition of familiar material, where verbal coding may be assumed to be involved.

Both groups obtained higher recognition scores on the familiar material. The normals were superior to the SSNs on recognition of common objects, but the SSNs were superior to the normals on recognition of unfamiliar material. An attempt is made to explain this interaction by postulating relatively intact stereognostic ability in the SSN group and by examining the role of verbal coding in the retention of information from stereognostic exploration. It is also suggested that apparently dissonant evidence on the tactual recognition of shapes by normal and retarded children is not incompatible with the present findings.

Hermelin & O'Connor (1961) report a difference in the performance of imbecile and normal children, matched for mental age, on a test of stereognostic recognition of unfamiliar shapes. They suggest that relatively intact stereognostic discrimination ability in imbeciles may explain this result. They further suggest that the unfamiliarity of the materials used virtually excludes verbal encoding and decoding in the task. Earlier they have stressed the improvements found in cross-modality tasks where verbal coding may be involved. This presumably implies that, in a situation where verbal coding is employed, the stereognostic recognition scores of both imbeciles and normals will improve. It is not clear whether these improvements will be relatively uniform or not.

A recent note by Medinnus & Johnson (1966) reports that no difference was found in performance on a tactual recognition task involving unfamiliar materials between normal children and retardates matched on mental age. It is important to note that in this experiment the retardates used were of mean chronological age 9·4 yr. (range 7–12) and of mean mental age 6·5 yr., i.e. they were children who would be classified perhaps as subnormal, but certainly not as severely subnormal or imbecile. Since Hermelin & O'Connor's tentative explanation of their results involves some neurological speculation, and since it is generally accepted that neurological damage is both more frequent and more severe in the SSN than in the SN (e.g. Clark & Clark, 1966), the two findings may not be incompatible.

The present study was undertaken with normal children and SSN adults, matched on mental age, to investigate levels of performance in tactual discrimination and recognition (1) using familiar materials, where verbal coding would be involved, and (2) using unfamiliar materials similar to those in Hermelin & O'Connor's study, where verbal coding would be, as far as possible, eliminated.

METHOD

Subjects. The subjects were 28 institutionalized SSN adults and 28 normal children. The two groups were matched for mental age. The SSN patients were a clinically heterogeneous group with a median mental age of 6 yr. (range 3:9 yr. to 8:9 yr.) on a Goodenough Draw-a-Man Test and median chronological age of 28 yr. (range 16–53 yr.). None of these patients suffered severe sensory or motor handicaps. The normals were children from an Aberdeen infant school, whose median chronological age was 5:9 yr. (range 5:6 yr. to 6:0 yr.).

Materials. The materials were of two types, namely 10 common objects (cup, comb, pencil, orange, book, spoon, shoe, brush, key, fork) and 10 Russian and Greek letters similar to those used in Hermelin & O'Connor's study. These were cut from a bakelite plastic, $\frac{1}{8}$ in. thick, and were 5 in. high. A screen, 24 × 24 in. with two hand-holes, was used to ensure that the materials could be explored tactually but not visually by the subjects.

Procedure. The groups were subdivided into four matched groups and two different treatments were applied. These were manual inspection and recognition of (1) common objects followed by letters, and (2) letters followed by common objects.

The stimuli were presented for 10 sec. each, with a time interval of 5 sec. between stimuli. The subject put his hands through the holes in the screen and the stimulus object was put into them. He was then instructed to explore the stimulus, name it if in the 'common objects' category and try to remember it. Immediately after five objects had been presented in this manner, a recognition test was given. The five inspection objects and five other objects from the same category (i.e. objects or letters) were presented singly and in random order. The subject was instructed to say whether or not he had felt any particular stimulus on the previous occasion. This was followed after a pause of 10 sec. by similar presentation and testing of the category. In the 'letters' condition, no instruction to name the stimulus was given.

RESULTS

The data are the number of common objects correctly named and the correct 'Yes' and 'No' responses to each of the sequences of 10 stimuli. Since all subjects named all of the common objects adequately, no difference was obtained in verbal encoding procedure. Presentation of the correct 'Yes' and 'No' scores presents a problem.

Table 1. *No. of correct responses for each sequence of* 10 *recognitions*

(A_1, object recognition; A_2, letter recognition; B_1, objects first; B_2, letters first; C_1, normals; C_2, SSNs.)

A_1				A_2			
B_1		B_2		B_1		B_2	
C_1	C_2	C_1	C_2	C_1	C_2	C_1	C_2
10	7	9	8	5	6	8	8
10	10	10	7	5	4	5	6
10	8	10	9	4	6	6	7
10	8	10	8	4	5	7	4
10	5	10	8	5	5	4	5
9	5	10	10	5	5	7	6
10	9	10	9	7	9	7	9
9	8	10	9	7	6	6	9
10	9	9	8	7	8	3	8
9	10	10	9	5	8	4	8
10	10	9	8	4	8	7	7
10	10	10	10	5	7	4	7
10	7	10	10	6	5	5	10
10	10	10	10	4	7	6	10

Hermelin & O'Connor suggest a transformation which converts the sum of correct responses to a deviation-from-chance score. This appears to have the effect of producing much more unwieldy scores without altering either the form of the distribution or the relations among the data comprising it. Each deviation score (x) may be obtained in a much simpler way than that outlined by those authors—the deviation score $x = 5{\cdot}5R - 27{\cdot}5 + 11{\cdot}0$, where R is the number of correct 'Yes' and 'No' responses, 27·5 is the chance score and 11·0 was the constant they added to make all scores positive. It was decided here to use R instead of x and thus simplify computation. Raw scores are presented in Table 1 and the analysis of variance summary in Table 2. In these, factor A is the materials variable (A_1 = common objects, A_2 letters), factor B is the order variable (B_1 objects first, B_2 letters first) and factor C is the subject category (C_1 normals, C_2 SSNs).

Table 2. *Summary of analysis of variance*

Source of variation	S.S.	D.F.	M.S.	F	P
Between subjects	124	55			
B	7	1	7	3·19	$> 0{\cdot}05$
C	0	1	0	—	—
BC	3	1	3	—	—
Error_b	114	52	2·19	—	—
Within subjects	365	56			
A	252	1	252	230	$< 0{\cdot}001$
AB	2	1	2	—	—
AC	52	1	52	46	$< 0{\cdot}001$
ABC	0	1	0	—	—
Error_w	59	52	1·13	—	—

Two significant effects were obtained: (1) Main effect for A. Inspection of the totals for A_1 and A_2 indicates superior performance on the common objects. (2) The AC interaction. Application of the appropriate t tests gives positive differences significant beyond the 0·01 level between the following pairs of means:

$$A_1C_1 - A_1C_2; \quad A_2C_2 - A_2C_1; \quad A_1C_1 - A_2C_1 \quad \text{and} \quad A_1C_2 - A_2C_2.$$

Objections may be raised to the appropriateness of the analysis of variance model for these data. However, a non-parametric analysis using the Kolmogorov–Smirnov technique produces the same results. These indicate that (1) each group achieves higher scores on common object recognition than on letter recognition; (2) normals are superior to SSNs on common object recognition; and (3) SSNs are superior to normals on letter recognition.

DISCUSSION

The superior performance of both groups on common objects is an expected finding and excites little comment. It is in keeping with established findings on the recognition of familiar *v.* unfamiliar stimuli. It also accords well with Hermelin & O'Connor's finding that verbal coding improves recognition on cross-modality tasks.

The materials × groups interaction is more interesting. An explanation has to be found for the superior performance of the normals on common objects and the superior performance of the SSNs on the letters.

The main difference noted on the common objects task was that the SSN subjects were inferior in their identification of objects not previously encountered. Both groups had optimal scores for the objects already explored, but the SSN subjects gave more 'Yes' responses to those not in the original five presented. Since no differences were found in ability to name the objects, the inferiority of the SSN group would appear to be related to inefficient storage and/or retrieval and/or decoding, as opposed to any encoding difficulty.

The superior scores of the SSN groups in recognition of unfamiliar stimuli may be due, as Hermelin & O'Connor suggest, to superior stereognostic recognition ability. It is also possible that some form of proactive interference is more powerfully affecting the normals than the SSNs in that the normals are more set towards remembering through verbal labels. This seems less likely in that no significant order of presentation effect was found. We would suggest that if such inhibitory effect were important, the decrement in performance on letter recognition would be greater in the B_1 condition (objects first), where the set would be towards labelling, than in the B_2 condition (letters first). Such a difference was not obtained.

The explanation of the inferred superior stereognostic ability of the older SSN group is difficult. Any physiological explanation can be only speculation on our part. But at least a plausible suggestion is that stereognostic discrimination function is controlled by centres which need not be involved in many types of cerebral insult associated with severe subnormality—Morgan & Stellar (1950) indicate that, in chimpanzees, stereognostic discrimination function depends upon the posterior 'parietal association' area, but may also have associated dependence upon subcortical centres. We suggest that, in those of our SSN subjects where brain damage may be severe, there will be some whose stereognostic discrimination centres are intact or less severely limited than would be expected from the level of damage associated with their intellectual deficit. Hence stereognostic discrimination function may be further developed than general intellectual function. Thus, although they are matched with normal children for mental age, their performance on tactual discrimination tests will be superior. Such a suggestion is supported by the minimal correlation found between mental age and stereognostic recognition performance in the SSN group ($r_s = -0{\cdot}05$).

Such reasoning does not apply to the retardates of Medinnus & Johnson's study; hence their findings are not comparable with those of Hermelin & O'Connor and of this present paper.

If this explanation of the SSNs' superior performance on unfamiliar letters is accepted, a further hypothesis is required to account for their inferior performance on common objects. We suggest that, where naming of the material is possible, information to be stored from stereognostic exploration is transformed and that this transformation depends heavily on verbal coding. In dealing with common objects where verbal coding is demanded, the normals have achieved higher recognition scores than the SSNs, owing to their more efficient remembering of this transformed information. As previously stated, we believe that it is in the retention, retrieval and decoding processes—as opposed to encoding—that the major differences between the groups occur. Further research might elicit whether this is an artifact due to the ease with which the common objects materials in this investigation could be coded.

We wish to thank the Director of Education for Aberdeen City, Miss Jean M. Peters, headmistress, and the children of Deeview Infants' School for their co-operation. We are grateful also for the co-operation of Dr H. S. Ross, Medical Superintendent, the staff and patients of Ladysbridge Hospital, Banff, and particularly for the advice and encouragement given by Mr David Clark, Consultant Psychologist, N.E. Regional Hospital Board.

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DEUTSCH'S MODEL APPLIED TO A CONDITIONAL MAZE

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Holding & Pritchatt have recently suggested that an experiment by Sheldon, in which rats learned to choose the more frequently rewarded of two paths to the same goal, is not a critical test of Deutsch's theory of behaviour since that theory can be shown to predict that certain schedules of training will establish temporary preferences for one of the paths. The results of an experiment with mice were offered as confirming this prediction. In the present paper it is argued (1) that Holding & Pritchatt's results are not entirely satisfactory; (2) that their experiment is probably not relevant to Sheldon's experiment; (3) that Deutsch's theory cannot explain the apparent stability of conditional learning.

Sheldon (1964) claimed to have shown that, in a maze where two paths of equal length led from a common start-box to a common goal-box, rats could learn to choose the path that more often led to reward. This was so whether choice of the 'incorrect' path was never rewarded, or rewarded half the time. It was suggested that the learning of problems of this kind—which it is convenient to call 'conditional'—cannot be explained by Deutsch's (1960) theory. Holding & Pritchatt (1967) have argued that these results *can* be explained by the theory, and they have described an experiment with mice that was designed to support their argument.

THEORETICAL CONSIDERATIONS

According to Deutsch's theory, information about the environment is stored in a network of 'links' through which, in accordance with certain rules, motivational energy passes to determine the animal's response. The reason for claiming that the theory predicts difficulty with the conditional problem is that the links representing the correct path and those representing the incorrect path will all be energized as long as they are connected to a set of links representing the common goal-box of the maze. (That this does present a difficulty for the theory is acknowledged by Deutsch, 1960, p. 121, in his discussion of an experiment by Hull.) Holding & Pritchatt suggest, however, that since the common goal-box may have offered a different appearance—and therefore been represented by different links—as the animal entered it from the two pathways, the experiment was not a critical test of the theory. It should be noticed, of course, that this argument does not help if there is a stage, once the animal is *inside* the goal-box, at which all the cues available are the same, whichever path the animal has taken. Holding & Pritchatt suggest (p. 85) that, even though there may have been such a stage in Sheldon's experiment, it is possible that the available cues were not represented by links. A more nearly critical test of the theory would be 'provided by a longer final path containing distinctive cues'. But for Holding & Pritchatt to speak of cues provided by the 'longer final path' as 'distinctive' is to beg the question. The difficulty is that Deutsch's theory does not allow us to say precisely what features of a particular environment will be represented by links. If it is possible to argue that in Sheldon's apparatus the goal-box cues were not represented, then, by the same token, it is possible to argue that the cues arising from a 'longer final path' will also be unrepresented.

Holding & Pritchatt put forward a second reason for saying that Sheldon's evidence is not critical. They quote Deutsch's Postulate 12 (Deutsch, 1960, p. 105), according to which each time a link is switched off its threshold is lowered by an amount 'proportionate to the intensity of excitation present in the link at the time'. They show how it follows from this postulate that if free trials are allowed after each of three different schedules of forced trials, there will, at least initially, be differences in choice of the correct path. It is important to make it clear that this postulate does not allow us to predict that, in general, rats will learn conditional problems. Indeed, as Holding & Pritchatt stress (p. 91), any preference established by a particular schedule will be temporary: after a long series of free trials, choice should again be unaffected by reward. It is being suggested, then, that Sheldon's results (which apparently showed that rats can learn a conditional problem) can be explained by arguing that the sequence of free and forced trials used in his experiment temporarily biased animals towards choosing the correct path.

Three questions arise from Holding & Pritchatt's arguments. In the first place, how clearly does the evidence from their experiment with mice confirm the predictions they make from Postulate 12? Secondly, how far are they justified in explaining the behaviour of the rats in Sheldon's experiment as the result of a temporary bias produced by the particular training procedure? Thirdly, is there evidence about conditional learning from other sources?

HOLDING & PRITCHATT'S EXPERIMENT

Holding & Pritchatt predict that their three schedules would lead initially to different levels of choice of the correct pathway. In Table 1 they present an analysis of variance to show that over the first 10 free trials the effect of schedules was significant, but no information is given about the significance of the differences between pairs of schedules. In particular, it is not clear whether the 'Alternation' schedule and what they call the 'Sheldon' schedule produced reliably different behaviour; Figs. 1–2 do not allow us to decide. Since Holding & Pritchatt predict that the 'Alternation' schedule should produce a preference for the *incorrect* path, and since they explain the fact that Sheldon's rats preferred the *correct* path as due to a bias introduced by the 'Sheldon' schedule, it could be argued that their experiment needed to show that these schedules produced different behaviour.

There is, in any case, something odd about comparing the 'Sheldon' group with either of the others. Each point shown in Figs. 1–2 (Holding & Pritchatt, 1967) represents the combined number of choices of the correct pathway for eight rats summed over two free trials (out of a maximum, that is, of 40). For the 'Block' and 'Alternation' groups the 20 free trials were given in a continuous series following 21 forced trials, and for these groups the greatest effect of the different schedules should have been on the *first* free trial. But for the 'Sheldon' group the 20 free trials alternated with forced trials throughout training, and this means that any bias introduced by this schedule should have increased with successive free trials, and should, presumably, have been at its greatest on the *last* free trial. This seems to make the analysis of the 'schedules' effect in Table 1 inappropriate.

RELEVANCE TO SHELDON'S EXPERIMENT

Even if Holding & Pritchatt's results were taken as confirming their predictions, it would not necessarily follow that their explanation of Sheldon's findings was correct. There are two important differences between conditions for Holding & Pritchatt's 'Sheldon' group and those under which Sheldon's animals were trained. In the first place, in Holding & Pritchatt's experiment choice of the incorrect path was never rewarded, whereas in his experiment it was rewarded half the time. No doubt it would be possible to extend their analysis of the effects of different training schedules to cover this relatively complicated case, but Holding & Pritchatt do not deal with this problem. Secondly, Holding & Pritchatt gave alternate forced and free trials to their 'Sheldon' group; but this must have arisen from a misunderstanding. Unfortunately, the procedure in Sheldon's experiment was not described as fully as it now appears would have been desirable, but there was nothing to suggest that animals 'alternatively chose and were forced', as Holding & Pritchatt seem to have assumed. The procedure (which was designed to ensure that at the outset rats were exposed to the contingencies associated with both pathways) demanded that rats met three successive performance criteria. Initially only *one* of the four daily trials (first, second or third) was free, the other being forced in such a way that the animal took each path twice. When an animal had made four successive correct choices on free trials, it was moved to a condition in which there were *two* free trials each day (first and second, or first and third). When it had again made four successive correct choices on free trials, it was moved to a condition where all *four* daily trials were free, and training continued until at least 11 out of 12 successive choices under these conditions had been correct. These two differences make it difficult to understand the relation between the 'Sheldon' condition and the condition in Sheldon's experiment.

But there is another reason for thinking that Holding & Pritchatt's results may not be relevant to Sheldon's. We have already seen that Postulate 12 does not allow Deutsch's theory to predict that conditional learning will be possible; certain schedules may bias the animal's responses, but a run of free choices will always be expected to even matters out. There is, however, a considerable amount of evidence from the experiment in question and from other experiments that this does not happen.

(*a*) In Sheldon's experiment, rats ended their training with a run of free trials over which they scored at least 11 out of 12 correct. Fourteen rats out of 15 managed this in an average of 66 trials, counting from the beginning of the experiment.

(*b*) In the first experiment reported in the same paper (Sheldon, 1964), six rats for whom one path was always rewarded and the other never rewarded learned to choose the correct path (to a criterion of seven out of eight successive choices correct) rapidly, and their training schedule was not complicated by forced trials.

(*c*) In the third experiment reported in that paper, rats were able to learn to choose one path to the goal when one signal was presented in the start-box and the other path when a different signal was presented. They learned more slowly than rats for whom the two paths led to different goal-boxes, but four rats reached the high criterion of at least five correct trials out of six on four successive days. This is a task in which Deutsch's theory predicts failure. Further related evidence is to be found in a more recent paper (Sheldon, 1967, Expt. 1).

These results seem to show that rats can perform consistently well on conditional problems. It could always be argued that performance in these experiments would eventually have deteriorated, but that argument, of course, could never be refuted.

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Holding and Pritchatt write:

Limitations on space preclude a detailed reply. However, it seems obvious that Sheldon's comments leave untouched the main points of the Holding & Pritchatt paper. It remains true that Deutsch's theory permits more sophisticated application than has been offered by his critics, that predictions for conditional learning will depend upon the sequence structure of training trials and that, in particular, the predictions for Block and Alternation schedules are sound. As for the 'Sheldon' schedule, this differs from the rather arbitrary procedure now described, but is consistent with Sheldon's (1964) earlier account: 'to ensure full and equal exposure to the two reinforcement schedules animals were given forced trials on both routes to balance the choices made on the free trials with which they were interspersed'. In either case, one can predict only that performance by the Sheldon group will lie somewhere between the Block and Alternation extremes, the exact position depending upon the sequence of responses on free trials, hence the demand for a significance test between the Sheldon and Alternation schedules is quite misconceived. It would be inappropriate here to attempt an analysis of the other cited cases of conditional learning.

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IMPRINTING AND THE FOLLOWING RESPONSE AS A FUNCTION OF AMOUNT OF TRAINING IN DOMESTIC CHICKS

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Two groups of domestic chicks were given 30 min. training with a moving stimulus approximately 14 hr. post hatch. Following behaviour was measured at 24-hourly intervals over 7 days, one group having a further 30 min. training each day. No differences in the amount of following was found between the groups though they both showed a steady decline in following over the 7 days of testing. Approach and free field responses were measured subsequently. The group which had the most visual experience with the training stimulus showed significantly more attachment responses. The results are discussed in terms of a perceptual learning model for imprinting and certain differences between following and approach are discussed.

The term 'imprinting' refers to the development of an attachment between a young bird and a particular stimulus configuration. It has been defined by Bateson (1966) as 'the process which restricts social preferences to a specific class of objects'. Imprinting has been measured in various ways; for example, as following or as approach to the imprinting stimulus (Sluckin, 1964).

A number of hypotheses have been offered to account for what has become known as the strength of imprinting. Salzen & Sluckin (1959) found that the amount of following shown by domestic chicks was related to the amount of initial experience during the training session. Other workers have suggested that the extent to which a bird subsequently prefers an object is related to the amount of following during the training session. Hess (1959) has gone so far as to formulate a law which states that the effectiveness of imprinting is a function of the effort expended by the bird in following the model (stimulus) during the training period. That this is an inadequate hypothesis has been pointed out by Bateson (1966) in his excellent review of the literature. He suggests that a more appropriate way to summarize the experimental findings is to postulate that imprinting effectiveness is dependent on the amount of visual contact which the animal has with the stimulus.

Sluckin & Salzen (1961) and Sluckin (1962, 1964), in pointing out that imprinting is the establishment of a preference, which implies that the animal can distinguish the stimulus to which it is imprinted from others, suggest that this is a process of perceptual or 'exposure' learning. On this basis, then, imprinting is not the learning to follow but rather a learning of the characteristics of the imprinting stimulus.

The learning of such characteristics will be a function of a number of different variables, but, as Bateson (1966) has pointed out, one such variable which will be important, and manipulable, is the length of visual contact which the animal has with the imprinting stimulus. In order to discriminate and so show a preference the bird must establish an engram or neural model of the stimulus. It is postulated, therefore, that the greater the animal's perceptual experience the greater its opportunity to learn the characteristics of the model and so discriminate between it and other stimulus configurations. The experiment reported in this paper was designed to examine the hypothesis relating attachment behaviour to perceptual experience.

METHOD

Subjects. Eggs of pure-bred Cornish Rock chicks were obtained from an attested commercial hatchery and stored for 5 days in a refrigerator. After this the eggs were held at 70–78° F. for 6 hr. prior to incubation in an electrically controlled forced-air incubator. Gottlieb (1961) has shown that treating eggs in this manner ensures that chicks hatching over a defined period are of the same developmental age.

When the chicks had dried off after hatching they were transferred, in darkness, to individual fibre-glass cages and kept in a constant-temperature room at 90° F. The birds were visually but not auditorily isolated from each other. Thirty-six chicks were randomly assigned to two groups, one experimental (E) and one control (C), each group containing 18 chicks.

Apparatus

Training. The training batteries, designed by Dr H. James, consisted of boxes 38 in. long, 18 in. wide and 24 in. deep, and fitted with lids containing a small one-way vision screen through which the chicks could be observed. The boxes were divided into two equal compartments by a transparent 'Perspex' partition. The floor was made of ¼ in. wire mesh under which fitted a sawdust tray, the interior walls and ceiling were painted a flat grey. A 50 W tungsten bulb was housed in a recess in the lid.

In one half of the box was fixed a rotating arm driven by a Crouzet synchronous motor and from this was suspended the imprinting model (stimulus) on a strand of wire. The model was a 2 in. diameter plastic sphere coloured half red and half white suspended 1½ in. from the floor of the battery. A timing device was used to automatically programme the movement of the model on a cycle of 1½ min. movement followed by a ½ min. no movement.

Testing

Following. Following was measured in a circular drum, 42 in. in diameter and 18 in. high. From a scaffold mounted over the drum a central arm, 24 in. long and pivoted at the centre of the drum, rotated. From one end of the arm was suspended the training model (red and white sphere) and from the other an 'irrelevant' model. This 'irrelevant' model was a 1⅝ in. cube of blue foam rubber. Both models were suspended 1½ in. above the floor. As with the training batteries the walls of the apparatus were painted a flat grey and the floor was a ¼ in. wire mesh mounted over a sawdust tray.

Following was defined as moving in the same direction as the model within an area of 3 in. on either side, or in front, and 9 in. behind it.

Approach. The approach response of the birds was measured in a runway 10 × 1½ × 1 ft., the walls of which were painted grey and the floor covered with black rubber matting. This facilitated cleaning and reduced the amount of skidding when the chicks moved quickly. Illumination was provided by fluorescent lamps mounted above the runway and behind a diffusing screen. At each end of the runway was suspended either the training or 'irrelevant' model. The models were changed to opposite ends of the runway for half of each group of birds in order to control for any position effect.

Free field measure

For this the training batteries were used with the 'Perspex' partition removed. Two strips of wooden beading, ¼ in. wide by ⅛ in. deep, were laid across the floor of the compartment to divide it into three equal portions. At one end was suspended the training model and at the other end the 'irrelevant' model.

Experimental design

The design for the first part of the experiment is shown in the following paradigm.

Day...	1 (of hatching)	2	3
Group E (experimental)	Train	Test, train	Test, train
Group C (control)	Train	Test	Test

This was repeated through 8 days, the experimental group were therefore trained on days 1–7, the control group on day 1 only, and both groups were tested on days 2–8. After this part of the experiment both groups were tested in the approach situation and then in the free-field situation.

Procedure

Ten batteries were available for training. The chicks were transferred from the animal house to the laboratory in their individual cages. Then, with the laboratory in darkness, they were placed in the batteries, one chick to each compartment. When the chicks had been placed in the batteries the light in each box was switched on so that the animal could see the model through the 'Perspex' screen. This was followed immediately by switching on the synchronous motor which through a timing device controlled the movement of the model. Each chick was given two 15 min. training sessions 12–14 hr. after hatching. These two training sessions were separated by 30 min.

Twenty-four hours after the initial training sessions the chicks were placed individually in the circular runway and the motor driving the central arm was switched on. The amount of time which the chick spent following the training model or the 'irrelevant' model was measured during a 5 min. test period. Group E (experimental) were then given a further two 15 min. training sessions. Both groups were again tested 24 hr. later for amount of following, after which group E received two more 15 min. training sessions. This procedure was repeated over 7 days. Animals in group E received a total of 3½ hr. of training whilst those in group C received only the initial 30 min.

On day 11 post-hatch the approach response of the two groups of chicks was measured in the 10 ft. runway. At one end of the runway was suspended the training model whilst at the other end the 'irrelevant' one was hung. Each chick was placed in the centre of the runway 5 ft. from each of the models and facing the side wall. Two measures of approach behaviour were taken: (1) latency—time to move 12 in. from the start position in the direction of one of the models; (2) time to reach within 12 in. of one of the models.

The data on approach behaviour relate therefore to the proportion of birds making an approach response and to the response latency.

On day 12 post-hatch the birds were placed in the batteries with the 'Perspex' partition removed, the training model being suspended at one end and the 'irrelevant' model at the other. Again this was counterbalanced so as to control for any position effect. The birds were placed individually in the centre section facing a side wall; the battery lights were then turned on. The position of the chick was recorded every 30 sec. throughout a 10 min. test period. Scores were assigned in the following manner: if the chick were in that portion of the compartment with the training model, +; in the centre section, 0; and in that portion of the compartment with the 'irrelevant' model, −.

RESULTS

Following behaviour

The mean time (in sec.) spent following the training model during the 5 min. test periods is shown for both groups in Fig. 1. It is apparent that both groups exhibit a following response and that this response wanes in the course of the experiment. A repeated measures analysis of variance based on a paradigm given by Winer (1962) was performed on the data and the results of this are given in Table 1. The two groups do not differ significantly in terms of the amount of the following which they show, though there is a significant decline in the time both groups spend following over the 7 days of testing ($P < 0{\cdot}001$). There is no significant interaction between the experimental treatment and the days of testing. Both groups showed significantly more following to the training model (that to which they were originally exposed) than to the 'irrelevant' model.

It seemed possible that the effects of additional training might manifest themselves not in the amount of following shown to the training model, but on the amount shown to the 'irrelevant' model. Since a discrimination test was employed, it is possible that any improved discrimination on the part of the experimental group might show up in terms of differences in response to the 'irrelevant' model. The group with the greater experience of the training model might follow the 'irrelevant' model significantly less. This would be evidence of the effect of additional training on the course of the following response. The amount of following shown by each group to the 'irrelevant' model is shown in Fig. 2. Again both groups show a steady decline in the amount of time spent following the 'irrelevant' model over the 7 days. An analysis of variance revealed that no significant differences existed between the two groups and that both groups show a significant decline in following the 'irrelevant' model ($P < 0{\cdot}001$). A summary of the analysis is given in Table 2.

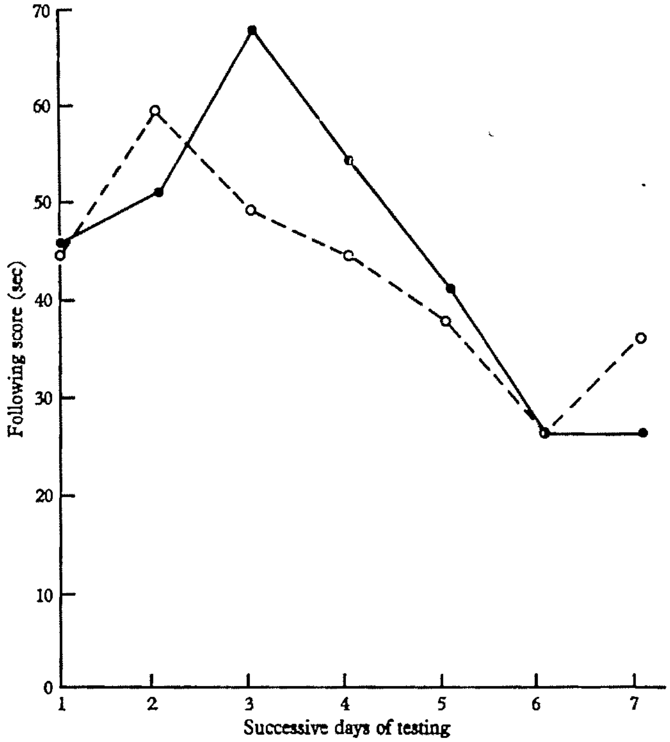

Fig. 1. Time spent following the training model over 7 successive days of testing. ——, experimental group: – – –, control group.

Table 1. *Summary of analysis of variance of following scores to the training model*

Source	S.S.	D.F.	M.S.	*F*	*P*
Between subjects	74092·99	35	—	—	—
A (treatment)	157·15	1	157·15	0 072	n.s.
Subjects within groups	73935·85	34	2178·58	—	—
Within subjects	195714·90	216	—	—	—
B (days)	31293·05	6	5215·51	6·71	< 0·001
A × B	6047·72	6	1007·95	1·29	n.s.
B × subjects within groups	158374·10	204	776·34	—	—

Approach behaviour

Seventeen of the 18 chicks in the experimental group approached the training model (defined as moving towards it at least 12 in.), whereas only nine chicks in the control group made an approach. Sixteen of the 17 experimental group chicks which made an approach to the training model approached to within 12 in. of it, whereas only seven of the control group chicks approached so far. These differences between the two groups are significant at $P < 0{\cdot}01$ on a χ^2 test. One chick from the experimental group and three from the control group approached the 'irrelevant' model.

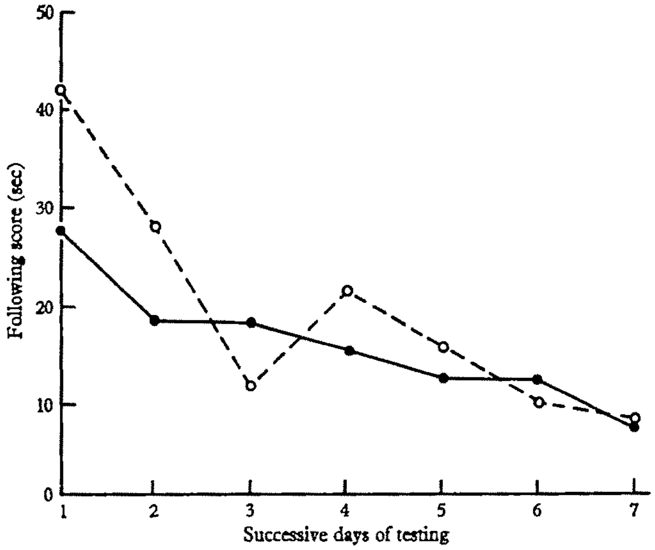

Fig. 2. Time spent following the 'irrelevant' model over seven successive days of testing. ——, experimental group; – – –, control group.

Table 2. *Summary of analysis of variance of following scores to the 'irrelevant' model*

Source	S.S.	D.F.	M.S.	*F*	*P*
Between subjects	26419·47	35	—	—	—
A (treatment)	941·15	1	941·15	1·255	n.s.
Subjects within groups	25478·33	34	749·36	—	—
Within subjects	79353·43	216	—	—	—
B (days)	17502·26	6	2917·04	10·09	< 0·001
A × B	2878·55	6	479·76	1·66	n.s.
B × subjects within groups	58972·62	204	289·08	—	—

A comparison of the approach latencies between the 17 experimental group and nine control group chicks revealed no differences on a Mann–Whitney *U*-test. The mean latencies were 38 sec. for the experimental group and 36 sec. for the control groups.

Free-field test

The results of this test are shown in Table 3. Each chick was assigned a score of +, 0 or − on each of 20 successive samples, the time between each sample being 30 sec.

The score for each animal could therefore vary between 0 and 20 for being in that portion of the apparatus with the training or 'irrelevant' models. The experimental group scored significantly more than the control group with the training model (Mann–Whitney U-test, $P < 0{\cdot}05$). There is no difference between the two groups in terms of their scores with the 'irrelevant' model.

Table 3. *Results of free-field test on control and experimental groups*

(Scores are assigned on basis of a chick's position in the apparatus when an observation is made. The maximum score in any category is 20.)

	Experimental group		Control group	
Chick number	+ scores	− scores	+ scores	− scores
1	17	1	2	0
2	18	1	0	0
3	19	0	5	5
4	19	0	14	0
5	6	11	20	0
6	18	0	20	0
7	7	0	9	0
8	18	2	20	0
9	19	1	7	0
10	20	0	12	2
11	20	0	9	5
12	16	1	14	0
13	0	17	18	2
14	20	0	12	0
15	20	0	0	9
16	12	7	0	0
17	18	0	16	1
18	19	1	0	17

DISCUSSION

Several studies have shown that the following response decreases over time. Fabricius & Boyd (1954) found that mallard ducklings cease to follow at about 10 days after hatching. Salzen & Sluckin (1959) reported that the following response of domestic chicks declined over 6 days of testing following the initial training experience. Similarly, Moltz & Rosenblum (1958), using Peking ducklings, found a decline in following over 15 successive days of testing. The results of this experiment are thus broadly in line with others regarding the decrement of the following response.

The experimental group which were retrained daily for 7 days received a total of 4 hr. 5 min. exposure to the training model; of this, 3 hr. 30 min. was in the training apparatus and 35 min. in the test situation. The control group, on the other hand, received a total of 1 hr. 5 min. exposure to the training model, 30 min. in the training situation and 35 min. in the test situation. There is no difference, however, in the discrimination shown by the two groups, measured in terms of the time spent following the 'irrelevant' model. Similarly, the strength of the following response to the training model is not enhanced by the training given to the experimental group on days 2–7 post-hatch. It appears that the crucial variable in this situation is the initial exposure; Salzen & Sluckin (1959) found that strength of following did vary with the duration of the initial training session. Once triggered, however, the following response appears to run its natural course and be controlled endogenously.

It has been suggested (Sluckin & Salzen, 1961) that the decrement in the following response is a result of the repeated presentation of the imprinting stimulus and is due to the animals becoming habituated or satiated with the stimulus configuration. However, this explanation appears inadequate in view of the differences between the two groups in the approach and free-field tests. If the decrement in following were due to the animals becoming satiated with the imprinting stimulus, one would expect that the experimental group with their much greater exposure to the training model would show a more rapid decrement in following; this, however, is not the case. Similarly, if the satiation were to the whole stimulus situation, the experimental animals which were given far more experience in the imprinting batteries should be less responsive in the free-field test than the control animals with only two previous encounters with this environment.

The perceptual learning hypothesis for the development of attachments gains some support from the results of the approach and free-field tests. An increase in the amount of visual experience with the training model leads to a greater proportion of birds showing attachment behaviour subsequently. This is complicated to some degree by there being no differences in approach latencies between the birds from each group which made approach responses. However, this may be indicative of large individual differences in the learning of the characteristics of the training model.

What would certainly seem to be the case is that following and approach, though both aspects of attachment behaviour, are quite different behavioural systems. Following, once triggered, appears to run its natural course irrespective of subsequent exposure, whereas the probability of a bird making an approach response is significantly increased by more extensive visual experience of the stimulus. It is as if following were a finely tuned behaviour requiring for the most part only a relatively brief initial exposure to trigger it, whereas approach requires that the animal have more opportunity to learn the characteristics of the stimulus. It should be remembered, however, that the following and approach responses were studied in different environments and this may be a relevant factor in accounting for the differences between the two measures. This possibility could be controlled for by carrying out the following experiment in the long runway, a method similar to that used by Jaynes (1956).

Both following and approach behaviours relate to the establishment of social bonds though they may well have different functions. Following may serve to ensure that the animal obtains the necessary experience in order to learn the characteristics of its species. It may also serve as a protective device in as much as a parent may ward off predators or take some evasive action which the young and relatively vulnerable birds, if imprinted, will follow. If this is the case it is not difficult to see why following should wane. Hinde *et al.* (1956) found that the responses of coots and moorhens decreased around 4–5 and 7–8 weeks respectively. The relatively short duration of following shown by domestic chicks may be related to domestication. If following serves as a protective device, then it is probably not so important in a domestic context and may no longer provide any selective advantage.

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LESS IS MORE: A STUDY OF LANGUAGE COMPREHENSION IN CHILDREN

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A task devised to study comprehension of the words 'more' and 'less' was given to a group of 15 children aged between 3:5 yr. and 4:1 yr. Another task, designed to assess comprehension of 'less' without reference to 'more', was given to the same group approximately 6 months later. The main finding (which holds for both occasions) is that only one subject gave consistently correct responses to 'less'. For the others it seemed that 'less' remained largely undifferentiated from 'more'. The children showed a marked tendency to make the same response, irrespective of which of the two words was used in the instruction. 'More' was dominant as the interpretation given to the undifferentiated pair.

The difficulty occasioned to the human intellect by relational terms has been evident, and to some degree acknowledged, for a very long time. The classical expression of the kinds of perplexity they can engender is to be found in Plato's *Phaedo*. While many of his difficulties may seem to arise only in the context of his general philosophical position, such evidence as is available suggests that some of his puzzlement—for instance, as to how there can be in Simmias at one and the same time both tallness and shortness—may find echoes in the thinking of young children today. It is at any rate clear that children find relational terms difficult to master; but we know little as yet about the precise nature of the difficulties and still less about how they are older than overcome.

The present study is restricted to one pair of relational terms: 'less' and 'more'. Previous studies of these have for the most part been conducted in the context of investigations of Piaget's conservation tasks (e.g. Beilin, 1965; Smedslund, 1966; Griffiths *et al.*, 1967) and have been carried out with subjects somewhat those whose responses are analysed here.

It is evident that 'more' and 'less' are used in many different situations and it seems advisable to begin by offering an analysis of some of the main distinctions that are to be recognized and that may prove relevant for developmental inquiry.

(*a*) The terms may apply to discrete units or to continuous quantities, i.e. the nouns which they qualify may be either count or mass nouns.

(*b*) The terms may be used in situations that are static in the sense that two entities are compared with one another at a given point in time; or they may be used in situations that involve comparison across an interval of time.

(*c*) When there is comparison across time, only one entity—or more than one—may be involved.

(*d*) The situation where two entities and two points in time are under consideration may be schematized as in Fig. 1. As the arrows indicate, six comparative judgements are then in principle possible.

(*e*) When there is comparison across time involving two entities, these may or may not be equal at t_1; and they may or may not have changed in quantity at t_2. If they change, one or both may do so. If both change, they may change in different directions, i.e. both may increase, one may increase while the other decreases, and so on. The changes may be related to one another, either directly or inversely (for instance, they might occur because of transfer from E_1 to E_2), or they may be independent. Finally, when there is change, the direction of change may or may not be in accord with the final state of the two entities relative to one another, i.e. E_1 may become more in passing from t_1 to t_2 but still remain less than E_2. In this case the comparison $E_1t_1 \leftrightarrow E_1t_2$ yields the judgement 'more', while the comparison $E_1t_2 \leftrightarrow E_2t_2$ yields the judgement 'less'.

(*f*) Where there is change, the person making the comparative judgement may or may not himself effect the change, i.e. he may be an active participant or a passive observer.

$$\begin{tikzcd}
E_1t_2 \arrow[r, leftrightarrow] \arrow[d, leftrightarrow] \arrow[dr, leftrightarrow] & E_2t_2 \arrow[d, leftrightarrow] \arrow[dl, leftrightarrow] \\
E_1t_1 \arrow[r, leftrightarrow] & E_2t_1
\end{tikzcd}$$

Fig. 1

The above analysis is evidently far from exhaustive. For instance, where more than one entity is involved the entities may or may not differ in respect of attributes other than quantity; and where there is change, attributes other than quantity may or may not also change.

The first study to be reported here had the following characteristics: (1) Discrete units were used. There was no investigation of continuous quantities. (2) There were always two entities involved in the comparison. (3) Both static and changing situations were used. (4) The entities were sometimes initially equal to one another and sometimes unequal. (5) The subject was sometimes asked to effect a change himself and sometimes merely to observe and judge it. Sometimes he observed the change as it actually took place, sometimes only the outcome of the change. (6) When change was effected by the experimenter, only one entity was altered. (7) The direction of change sometimes accorded with the final state of the entities relative to one another and sometimes did not.

In a second study, subjects were asked to deal with one entity only and to effect a change (in this case, a decrease) in its quantity.

METHOD

Subjects. The subjects were 15 children (nine boys and six girls) between the ages of 3:5 yr. and 4:1 yr. at the time of the first study. All subjects were taking part in a longitudinal study of the development of cognitive and linguistic skills—a study of which this investigation forms a part.

Material. The material used consisted of two cardboard apple-trees with six hooks in corresponding positions on each tree, and 12 cardboard apples which could be hung on the trees. The trees could swivel round, when this was desired, so that the subject could not see the experimenter attaching the apples to them. When the numbers on the two trees were made equal by the experimenter the spatial distributions of the apples were always the same.

First study

The subject was asked questions about eight different stimulus situations. The first set of questions involved the word 'more'. Questions of the same form, but using 'less', were asked on a subsequent occasion a day or two later.

(*a*) In three cases (questions 1, 3, 8) the subject had to make a comparative judgement in respect of a situation of static inequality. In question 1 the subject was himself first asked to produce this situation. He was shown two blank trees and asked to put more apples on one tree than on the other. If the child chose to leave one tree empty he was left free to do so. In questions 3 and 8 the subject was (theoretically!) a passive observer of situations presented to him by the experimenter. He was asked: 'Does one tree have more (less) apples on it than the other?' If he answered 'yes' he was asked: 'Which tree has more (less) apples?'

(*b*) In question 2 the subject was asked to judge a situation of static equality presented by the experimenter. The wording of the questions was the same as for questions 3 and 8, but if the subject denied that one tree had more apples he was further asked: 'Is there the same number of apples on each tree?'

(*c*) In questions 4 and 6 there was a situation of initial equality which was then changed by addition or subtraction. (Given initial equality, it obviously follows that the direction of change must accord with the final state.) In question 4 the subject was passive. The experimenter presented the two trees with equal sets of apples already in position, then held up one (or two) additional apples and asked: 'If I put this apple on this tree, will one tree have more apples than the other tree?' If the subject answered 'yes' he was next asked: 'Which tree will have more apples?' The addition was then made and the subject was asked: 'Does one tree have more apples on it?' As before, if he said 'yes' he was asked which one. In the case of 'less' the procedure was the same except that the experimenter removed one or more apples from one of the trees instead of adding. In question 6 the subject was active and was asked: 'Make it so that there are more (less) apples on this tree than on this one.'

(*d*) In question 5 the subject was presented with a situation of initial inequality and was asked to make a comparative judgement as before. One or more apples were then added by the experimenter to the tree which initially had less (or removed from the tree which initially had more), the change thus brought about being always too small to reverse the direction of the initial difference. Thus the final state did not accord with the direction of change. The subject was then again asked for a comparative judgement.

(*e*) In question 7 the subject, having been presented with initial inequality, was asked to reverse its direction. The instruction was: 'Now make it so that there are more apples on this tree [lesser number] than on this tree.' Or else: '...so that there are less apples on this tree [greater number] than...' Finally the subject was again asked for a comparative judgement on the outcome of his own efforts.

For all questions except question 1 there were several trials involving different numbers of apples. The numbers used on a given tree ranged from one to five and the differences between sets ranged from one to four.

Supplementary study

The second study was carried out (with the same subjects) approximately 6 months after the first. The aims were: (i) to see if there had been change in the typical response patterns over this interval of time; (ii) to check the possibility that in the first study the responses to 'less' questions had been influenced by the fact that they had been preceded (though not on the same day) by questions involving 'more'.

This time only one tree was used. It was shown to the subject with a number of apples already on it and he was asked: 'Make it so that there are less apples on the tree.'

There were two stimulus situations: (*a*) four apples on the tree, two lying on the table; (*b*) three apples on the tree, three lying on the table. Each of these was presented twice to the subject, the order of presentation being: *a*, *b*, *a*, *b*. After the subject had acted in response to the instruction, he was asked: 'Does it have less apples on it now?'

No reference to 'more' was normally made in this study, but in the case of two children who had shown uncertainty in their responses the experimenter added at the end of the session the request: 'Make it so that there are more apples on the tree.'

RESULTS

First study

One child gave very few responses throughout the test. Others occasionally failed to respond or refused to continue through all trials. Most of the following report is based on evidence from 14 subjects.

(1) The most consistent and striking finding is that, in the great majority of cases, responses gave no indication that the children were differentiating 'less' from 'more'. Questions containing the word 'less' were answered in the same way as questions containing 'more', i.e. in a way that would have been correct for 'more'. This was evident in the first task set, where the child was given two blank trees and told to put more (or less) apples on one than on the other. One child in the case of 'more', and two children in the case of 'less', put equal numbers on the two trees. The others successfully made them unequal. But in the case of 'less' only two subjects then correctly indicated which tree had less, the rest choosing the tree with the greater number as they had done previously. It is worth noting that four subjects left one of the trees empty and still chose the other as having 'less', which would seem to indicate that, for them at least, difficulty in counting or in recognizing the greater number was not the source of the trouble. (It should be observed that some of the children were able to count sets of the sizes here used; but there was no evidence of a relation between this ability and success in differentiating 'less' from 'more'.) The typical pattern is illustrated in Table 1 by a detailed analysis of responses to question 3. In this task a few children completed as many as seven trials with 'more' and six trials with 'less'. But others would not continue for so long. The total number of responses available is 69 for 'more' and 55 for 'less'.

Table 1. *Analysis of responses to question* 3

	Yes	No*	Same	Total
Does one have more?	65	3	1	69
Does one have less?	55	0	0	55
	Correct choice	Incorrect choice	No choice	Total
Which one has more?	63	5	1	69
Which one has less?	15	40	0	55

* Subsequently changed to yes.

On 40 trials, then, the child, having judged that one set contained less apples than the other, proceeded to choose, as the one that had less, the one that in fact contained more. The 15 correct choices of the tree with less were made by five children, of whom only one was correct on all presentations attempted. A further two children were correct in more than half of the responses they made.

The general finding that the children showed a marked tendency to interpret 'less' as synonymous with 'more' was confirmed repeatedly in the different tasks used. This was so consistent a finding that further data relating to it will not be reported in detail, except in the case of question 4, where the theoretical interest of the comparison between predictive and verificatory responses seems to justify a full presentation of the figures.

Only one child answered 'less' questions with consistent success. She responded as follows to the initial request to put less apples on one tree than the other. Subject: 'More on that one?' Experimenter: 'No, less.' Subject: 'More?' Experimenter: 'No, less.' Subject: 'Oh, just a little! About fou—...three!' She proceeded to put five apples on one tree and one on the other.

(2) When the children were presented with situations of initial equality and asked whether one tree had more (or less) apples on it than the other, three subjects denied on every trial attempted that one had more, and likewise consistently denied that one had less, responding to the question by a direct 'no', a shake of the head or statements like 'They're the same', 'No, none has' or 'No, both have the same amount.' A further six children gave evidence of denial of inequality, if statements such as 'Both of them', 'Both the trees', 'That one does an' that one', 'They two ones', 'Each tree', 'These two have' are accepted as a form of denial, i.e. if one admits as denial any refusal to select one tree rather than the other. Of the six children who used statements of the above kind, two, at one point or another, spontaneously asserted that the sets of apples were 'both the same' or 'both the same number', and all answered 'yes' to the question: 'Is there the same number of apples on each tree?' However, it is to be noted that three of these children gave answers of this kind only in response to the question: 'Does one tree have more...?', whereas in answer to '...less...?' they simply chose one of the trees. The other three responded to 'Less?' in the same way as they did to 'More?' using, once again, expressions like 'On each tree' or 'Both the trees'.

Table 2. *Analysis of responses to question* 4

	Correct choice	Incorrect choice	No reply	Total
Which tree will have more?	59	7	2	68
Which tree has more?	57	10	1	68
Which tree will have less?	19	33	3	55
Which tree has less?	16	39	0	55

(3) Question 4 required the subject to predict which tree would have more if the experimenter were to add either one or two apples (starting from a position of equality) and then to verify the prediction by making a comparative judgement after the addition had been made. As usual, a corresponding question was asked for 'less'. Prediction proved to be in general no easier than verification. With one single exception, all responses obtained to the general questions, whether predictive or verificatory (i.e. 'Will one tree have more (less)...?' and 'Does one tree have more (less)...?'), were affirmative. As usual, the questions asking 'Which tree...?' were more difficult. The responses to these are given in Table 2. It will be observed that the differences between prediction and verification are negligible.

When, in question 6, the child had himself to effect the addition or subtraction there was considerable scope for variation in the non-verbal responses. The subject might add to one or to both trees—and in the latter event might end with the numbers equal or unequal. He might also in principle remove apples, but this was done by only one subject—the child who was consistently successful with 'less' questions. By far the most common response, whether for 'less' or for 'more', was to add to the tree indicated; but a few children added to both, and it then occasionally happened that equal collections resulted. This equality might or might not be recognized as such by the child. As in other questions, subjects could for the most part correctly indicate which tree had more when there was inequality, but when asked which tree had less, they again showed a marked tendency to choose the tree which had more.

(4) When the situation was introduced (question 5) in which there was conflict between the act of adding (or removing) apples and the final static comparison of the two sets (i.e. when the tree to which more had been added was nevertheless the tree which had less at the end), four children who could successfully handle the initial static comparison in the case of 'more' became quite inconsistent in their replies, which suggests that the conflict was confusing to them. A further six, however, who were consistently correct in saying which tree had initially more apples continued to make the final static judgement correctly. The other children were inconsistent in both situations.

In the case of 'less', seven children consistently answered the initial question by choosing the tree which had more. They were all still consistently wrong after apples had been removed. Indeed there was more consistency from trial to trial here than in the questions involving 'more'. This at first sight is curious. But it should be noted that in this situation there are two possible sources of error which may reinforce one another and lead to the same wrong response. Suppose that the child initially chooses the tree with more apples as having 'less'. He then sees apples being removed from it—and continues to choose it as having 'less'. Is this because he bases his response on the final static comparison and so judges it still to have less because he is treating 'more' and 'less' as synonyms? Or is it that he has some grasp after all of 'less' in the sense of reduction in quantity? In the latter case he might choose the tree from which apples had been removed instead of basing his judgement on the final size of the two sets. If both of these processes were occurring, then the fact that 'less' questions produced more consistency of response than 'more' questions in this situation might be explained.

(5) A further task (question 7) required the subject to 'Make it so that there are more apples on this tree [lesser number] than on this tree'; and to act similarly, with appropriate reversal, for 'less'.

Here, once again, the subject may alter one or both trees by addition or subtraction, to achieve correct or incorrect results. Once again, most responses consisted in adding to the tree indicated, whether the request was to make it more or less. But many children made alterations that differed in type from one presentation to the next, and there was in general much inconsistency. Only three children were consistently correct with 'more', while only three were consistently incorrect. In the case of 'less', even the child who usually managed 'less' questions successfully showed some fluctuation. Once she succeeded by removing all the apples from the tree indicated, and once by adding to the other tree. But on a third presentation she added to both trees and ended with equal numbers, while on a fourth she added to both but ended with more apples on the tree which was supposed to have less.

The task, then, was a very difficult one for the children. It gave further confirmation to the view that 'less' was generally regarded as equivalent to 'more'. One child made this explicit. Asked to make three less than two, he said: 'But it *is* less on that tree.'

Supplementary study

Of the 14 children tested, seven unhesitatingly added more apples to the tree on each of the four presentations. Two subjects removed apples from the tree on the first presentation, but thereafter added apples each time. There were three subjects who, on at least one occasion, removed apples and then put some back again (replacing either less, the same number or more than they had previously taken away). Two of this last group of children made remarks which referred explicitly to 'trying' to make it less but not considering that the attempt had been successful. The third removed all the apples, said this had not made it less, proceeded to hang on all six available apples and declared that she had made it less now. It is possible that this was a reference to the set of apples on the table; and it may be that the three subjects who removed and then replaced apples were in fact shifting the focus of their attention from the apples on the tree to those lying beside it. One child responded to the initial instruction by saying: 'How can I?' and then made no further response to urgings by the experimenter that he should try. At one point he said: 'Take them off, you mean?' but then made no move to do so. However, he readily added apples when the experimenter finally tried replacing 'less' in the instruction by 'more'. Clearly for this child 'less' and 'more' were no longer synonyms, but the opposition of the two did not seem to be effectively established; and the perplexity so notably absent elsewhere in the findings did here show itself. The one child who had made a consistent distinction between 'less' and 'more' on the first testing was again the most successful in this second study. She removed one or more apples on each presentation, though even she began by adding an apple on one occasion.

Table 3. *Distribution of responses from children in the supplementary study*

Response	Instruction: 'Make it less'
Addition	41
Subtraction	6
Addition followed by subtraction	1
Subtraction followed by addition	4

From these children, 52 responses in all were obtained. The distribution is shown in Table 3.

DISCUSSION

It does not seem intuitively surprising that the word 'less' should come later in development, both in comprehension and production, than the word 'more', but this does not diminish the importance or the difficulty of trying to explain why it should be so.

A first obvious suggestion is that the difference is related to frequency of usage in the language. Both 'more' and 'less' have been reported to occur among the first 500 words in frequency counts in English (Thorndike & Lorge, 1944) and the same is true of their counterparts in French, German and Spanish (Eaton, 1940). However, the actual number of occurrences of the two words in the Lorge magazine count was 8015 for 'more' and 1357 for 'less'. In any event, this word-count evidence does not dispose of the question of whether in everyday verbal interchange between children and adults the frequency of usage of 'more' and 'less' differs significantly. It seems very likely that it does.

On the other hand, if the issue were merely one of relative familiarity with the terms, a finding different from the one reported here might be expected. If the children had rarely heard the term 'less' they might reasonably be expected to show perplexity when given instructions containing it—or at any rate to interpret it in a diversity of ways. What we have to account for is that they so commonly and with so little sign of hesitation interpret it as a synonym for 'more', sometimes making this interpretation very explicit (cf. p. 467). In other words, the children respond as if they know the word 'less' and as if they know that it refers to quantity. (If they had not appreciated this reference they could have tried responding by, for instance, rearranging the configurations, at least in the active tasks; but nothing of this kind was ever observed.) What seems to occur is that 'less' is understood to refer to quantity but that it remains largely undifferentiated from 'more', with 'more' as the consistently dominant interpretation for the undifferentiated pair.

So far, then, there are two distinguishable facts to account for: the lack of differentiation and the dominance of 'more'. The second of these can perhaps be related to a phenomenon of some generality. But first it may be worth considering the more special and limited question of the kind of context in which the word 'more' is normally first encountered by children. In the introductory analysis of situations in which 'more' and 'less' occur it was pointed out that these terms are regularly used both in static and in changing situations. It seems likely, however, that 'more' is first comprehended—and produced—by children in contexts where one entity changes. A quite young child can commonly say 'more' when he wants food in addition to what he has previously been given and it seems credible that this is ontogenetically the term's most primitive context. Notice that the word 'less', if heard by the child in this context, might well carry some such meaning as 'more, but not so much more'. One adult might say to another: 'That's too much. Give him less.' Whether this has any connexion with the findings reported here, however, it is at present impossible to say.

The more general issues raised by the apparent dominance of 'more', and other questions concerning the interpretation of the results, cannot be discussed apart from some consideration of the concept of a dimension, both with respect to its logical or formal properties and to certain features of the ways in which adult human beings appear to handle it. Though adults do not ordinarily fail to differentiate the terms that refer to opposite dimensional directions, they do not on the other hand necessarily —even normally—treat them as 'mirror-image' counterparts of one another, which is what the logic of the matter might be held to demand. On the contrary, as has been widely recognized, one term seems often to possess a 'positive' or dominant quality. The linguistic and psychological issues raised by this are of great complexity and cannot be discussed here; but it is evident that in the long run consideration of the dominance of one word from a pair of opposites in the speech of children cannot be separated from attempts at elucidation of the relations which obtain in the adult language. Where 'more' and 'less' are concerned, it is clear that 'more' is, for adults as for the children studied here, the normally dominant term.

So far as the logic of the matter goes, when order along a dimension has been established, the relation between any two entities, A and B, may, of course, be expressed in the conventional notation as $>$ or $<$ or $=$. Then, given no inversion of the order of A and B in the relational expression, all three relations are incompatible.

If 'less' and 'more' are treated as synonyms, this implies that the incompatibility of $(A > B)$ and $(A < B)$ is not being acknowledged—at least through the use of these terms. It does not follow that the children entirely fail to apprehend it, or even that they do not have available to them some other means of expressing the distinction. But it is also necessary to consider whether recognition of the incompatibility of $(A = B)$ with each of the other relational terms is manifested. That this is not always so is shown, in the first place, by the occurrence of instances where a child allows that one tree has more (or less) apples than the other, and immediately afterwards that the two have the same number. (Here, once again, there is the problem of relating the child's language to his conceptual structuring of the situation.) But further it is necessary to consider in this connexion a feature of the results not yet discussed: namely, the occurrence, in situations where the two sets were in fact equal, of statements such as 'Both the trees' and 'These two have'. Responses of this kind never occurred when the two sets were unequal, so they may therefore presumably be taken as evidence of a recognition of equality, i.e. as a means of expressing a judgement of equality. This supposition is further strengthened by the fact that subjects who gave answers of this kind occasionally spontaneously used the normal expression: 'Both the same'; and that they consistently then went on to answer 'yes' to the question: 'Is there the same number of apples on each tree?'

If answers to the effect that 'both have more' or 'both have less' are used to express judgements of equality, it is necessary to ask what this implies about the functioning of the words 'more' and 'less' in the child's language. There are at least two possibilities. One is that they have, in this case, their normal comparative role, but that each of the entities in question is being compared implicitly with a third, which is serving as standard. The child might be saying implicitly: 'Both have more than the number lying on the table', for instance. But then one might expect sometimes to get this kind of response when the sets were unequal.

The second possibility is that the words in this context are functioning virtually as synonyms for 'same'. Yet it is quite clear from other evidence—evidence, for instance, that the children can correctly judge which of two unequal sets has more, or that they can successfully change two equal sets so that one has more than the other—that the distinction between 'more' and 'same' is not one which they are incapable of making.

At this point some further consideration of the notion of incompatibility may be relevant. Lyons (1963) says: 'It seems evident that the semantic relation of incompatibility plays a vital part in both the learning and the use of language' and he goes on to suggest that: 'Two units that are incompatible may quite reasonably be said to be closer or more alike in meaning than two other units that are "merely" different in meaning....This holds for antonymy. It is far from self-evidently absurd to say, for instance, that *wet* (in English) is closer in meaning to its "opposite" *dry* than it is to, say, *cold* or *rich*.' But though Lyons distinguishes gradable from non-gradable antonyms he does not consider in detail how the conditions of semantic imcompatibility may differ in the two cases. For the purposes of this discussion it seems important to do so. Consider, therefore, the following two statements:

(i) If x and y are terms which refer to incompatible absolute attributes (e.g. non-gradable antonyms) then x and y cannot, simultaneously and without qualification, be predicated of one entity, A.

But on the other hand:

(ii) If x is a term which refers to an asymmetrical (gradable) relation, then it cannot without incompatibility be reciprocally predicated of two entities, A and B. That is, it cannot be maintained both that $A > B$ and that $B > A$—or that A and B are 'both more' (than one another).

So an interesting inversion arises. A cannot be simultaneously both red and green, though A and B may very well both be red. On the other hand, A and B cannot both be more (than one another) though either of them singly can be simultaneously more (than a second entity) and less (than a third).

The situation is further complicated by the fact that terms which refer to symmetrical relations are used in ways which resemble absolutes in certain respects and asymmetrical relations in others. We can speak of A and B as 'both the same', it being then generally assumed that we are comparing one of them with the other; also A can be simultaneously the same (as B) and different (from C)—or even, of course, the same as B in one respect and different from B in another.

Notice that where the relation is symmetrical the relational word in English may either come between the terms that are being related or follow them. Thus 'John and James are brothers' is normally taken to mean that they are one another's brothers and hence to be equivalent to 'John is the brother of James' (and 'James is the brother of John'). It is as if we could say: 'A and B have more', meaning 'A has more than B'—and indeed there would seem to be no logical reason why this should be excluded, since the need to have regard to order in the case of asymmetry could perfectly well be handled by distinguishing semantically between 'A and B have more' and 'B and A have more'.

However, 'both have more' could not satisfy this requirement; and it is in any event clear that this is not what the children are doing. In summary, (*a*) by treating 'more' and 'less' as synonymous, they fail to manifest in this situation recognition of the incompatibility between $A > B$ and $A < B$; (*b*) by using some statement to the effect that 'both have more' as a means of expressing a judgement of equality, they fail to manifest in this situation recognition of the incompatibility between $A > B$ and $B > A$.

The twice-repeated phrase 'in this situation' is important. The investigation reported here forms only a small part of a much more extensive study of the cognitive and linguistic development of this group of children. Attempts at full and systematic interpretation of the findings concerning 'more' and 'less' will ultimately depend on the ways in which they are found to relate to other observations made in a wide range of different performance situations (see Donaldson & Wales, 1968, in press) and to the results of experimental intervention designed to throw more light on the nature of the developmental processes involved. It seems clear that it is only through the use of a research strategy of this kind that progress towards an adequate theory of cognitive development can be made.

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A NOTE ON THE PREDICTIVE VALIDITY OF THE MPI AS TESTED BY A SEMANTIC DIFFERENTIAL TECHNIQUE

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Items in questionnaires purporting to measure personality are commonly affected by motivational fluctuation, but their conceptual generality should remain constant. In this event a questionnaire referring to typical modes of behavioural response, such as Eysenck's MPI, should predict a semantic appraisal of the self-image after a lapse of time. In relation to this, the present findings are of interest.

The MPI was administered 5 months prior to a semantic differential assessment of the self by 24 undergraduates, and the results for the extreme scorers are compared in Tables 1–2.

The dynamic factors of potency, activity and stability are significant for extraversion and that of evaluation for neuroticism. For stability, the variance for interaction is on the borderline of significance, and this is reflected in the E–I mean differences at high levels of N. This suggests that the effect of high N is to emphasize the stability pattern characteristic of each temperament—flexible impulse in extraverts and inhibitive restraint in introverts.

Table 1. *Mean scores for self-appraisal of the selected groups in the Maudsley Personality Inventory and Osgood's semantic differential scales*

(*Evaluation*: hopeful, unselfish, skilful, beautiful, successful, meaningful, important, attractive, heavenly, wise. *Potency*: hard, strong, prohibiting, large, humorous. *Activity*: active, hot, fast, effortless. *Stability*: guessing, balanced, rash, loyal, obstinate.)

	Scales named according to high scoring end					
	Group definition sigma scores					
	MPI N	MPI E	Evaluation	Potency	Activity	Stability
Group means (*n* = 6 in each)						
N− E	−0·90	+0·97	45·3	22·8	20·5	22·0
I	−1·00	−0·75	49·8	17·8	16·7	21·0
N+ E	+0·88	+1·22	42·7	24·0	20·5	23·2
I	+0·95	−0·97	40·2	17·3	15·1	20·5
Size of diff. significant at						
$P < 0·05$	—	—	5·9	2·9	2·9	2·4
$P < 0·01$	—	—	8·7	4·2	4·3	3·5
Rho with E	—	—	−0·13	+0·74***	+0·65***	+0·64***
N	—	—	−0·35*	−0·13	+0·06	−0·24

Significance level (one-tailed test): * 0·05, *** 0·001.

The mean differences and correlation for self-evaluation are less pronounced but support the usual finding that self-esteem tends to be lower in maladjusted people. These subjects, however, were all successful in their university careers and a clinical interpretation of N is not appropriate. A more useful approach is possible if an N score is taken to indicate the facility with which the individual ceases to orientate to a repeated stimulus, i.e. the number of trials to a habituation criterion. Using Taylor's Manifest Anxiety Scale, Koepke & Pribram (1967) obtained a correlation of +0·51 (0·01) between TMAS and the number of trials before there were three successive failures to orientate to an auditory stimulus. This slowness to habituate in high TMAS scorers may have significance for the lower mean scores for the N+ groups here. Characteristic arousal subsidiation patterns affect the dynamics of behaviour in terms of reaction time and intensity, and these are important variables in interpersonal perception. Assuming social feedback to be largely operative in the building up of self-esteem, it would seem from these results that low N-scoring introverts are most favourably endowed and high N-scoring introverts least so, in the temperamental factors which are important for self-acceptance.

Table 2. *Analysis of variance relating to the means in Table* 1

	Source of variance				
	E	N	E×N	Error	Total
Sums of squares					
Evaluation	6·0	228·2	73·5	706·3	1014·0
Potency	204·2	0·7	4·1	169·0	378·0
Activity	126·1	3·4	3·3	175·2	308·0
Stability	80·7	1·5	16·7	113·7	212·6
D.F.	1	1	1	20	23
Mean sum of squares					
Evaluation	6·0	228·2	73·5	35·3	—
Potency	204·2	0·7	4·1	8·4	—
Activity	126·1	3·4	3·3	8·8	—
Stability	80·7	1·5	16·7	5·7	—
F ratios					
Evaluation	0·17	6·46**	2·08	—	—
Potency	24·17***	0·08	0·49	—	—
Activity	14·39***	0·39	0·38	—	—
Stability	14·19***	0·26	2·93	—	—

Significance level (one-tailed test): ** 0·01, *** 0·001.

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CRITICAL NOTICE: THE ANALYSIS OF HIGHER NERVOUS ACTIVITY IN MAN

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Higher Cortical Functions in Man. By A. R. Luria. London: Tavistock. 1966. Pp. xvi + 514. 126*s.*

Human Brain and Psychological Processes. By A. R. Luria. New York and London: Harper & Row. 1966. Pp. xix + 587. 115*s.*

The organization of brain mechanisms may only to a limited extent be inferred from the behaviour of the intact organism. Further information derives from instances in which brain functions disintegrate, revealing through their impairment the nature and interrelationships of the component operations which interact to guide total behaviour. Focal cerebral lesions eliminate or downgrade one or more of these component processes. Subsequent behaviour is compounded of the performance that results and of reorganization of behaviour adaptive to the new circumstances. If the behavioural deficits are appropriately analysed, and the component processes assigned to their respective cerebral loci, it should ultimately become apparent how the nervous system is wired up. In the face of a century of effort in this direction, this expectation has by no means been realized.

Neuropsychological findings are customarily held to be weakened by uncertainties as to the localization of cerebral lesions in humans during life. Observer bias may influence diagnostic classification where this cannot be validated by direct anatomical inspection. Clinicians concentrate on the deficit they expect to find and disregard other aspects of cognitive functioning. In truth, the behavioural impairments are more general, and disease processes more widespread than usually reported. Though such objections are often justified, their motive force is usually adherence to the discredited doctrine of mass action (Lashley, 1929) as a model for cerebral organization, maintained by dint of ignoring large segments of the literature (Reitan, 1966; Smith, 1966). The work of Luria constitutes an impressive segment of this literature. While open to the above criticisms, nevertheless it retains outstanding significance, and its really important limitations are of another kind altogether. Had Luria presented fully documented case protocols, had he systematically explored all cognitive areas of each of his illustrative cases, his conclusions might well have remained essentially unaltered. Nevertheless, they could be misguided. The crucial question is: Did he appropriately analyse the disordered behaviour? Did he look for impairment of physiologically valid processes, or of mere man-made abstractions? If he asks the right questions, it is a matter of time only before, by sheer weight of case material, the correct answers become apparent.

The 'localizationist' approach to neuropsychology is often mistakenly regarded as postulating 'pure' deficits, limited to one particular activity, such as reading ('pure alexia'). Thus the concept of an isolated reading disorder is often challenged by the argument that, if sufficiently subtly tested, the patient will be found to suffer from other deficits as well (Bay, 1964). This misses the point at issue. It is not purity as a principle that is at stake, but the concept that a focal lesion may affect one function to an extent out of proportion to that with which the rest are affected, while lesions elsewhere spare that one disproportionately and affect others ('double dissociation'). If reading performance bears the brunt of disease, that is of significance even if other milder disabilities co-exist. The problem as regards pure alexia is the concept that there could possibly exist a disorder affecting reading *in toto*, while sparing all other performance. This assumes that reading is a unitary activity which is focally represented in the brain and in some general way impaired by the lesion. Luria makes no such assumption. He recognizes that reading is a learned skill, involving a variety of operations which cumulatively become available to the child during cerebral maturation, up to the point when he is potentially able to learn and has achieved reading readiness. As Luria points out, processes involved in reading may be variously localized, and he makes guesses at which those processes are, and where in the cortex their neural substrates are located.

Luria's model of brain organization provides for a limited number of very generally applicable processes, each characteristic of a major cerebral territory. Within a given area, a large variety of different types of input are subjected to the basic process. A limited focus of damage within the area might disconnect a particular type of input, at a particular level of analysis, from this processing, giving rise to a selective deficit. As a model this is far more promising than either the diffuse or the mosaic notions of representation of function, but ultimately its explanatory value will be a function of the aptness with which the hypothesized overriding processes are specified.

As regards nature of cerebral processes, Luria draws heavily upon the insights of Sechenov (1878) in categorizing the integrative function of the cortex into that making for 'simultaneous synthesis', of multiple coincident stimuli, and that making for 'successive synthesis', an ineptly named process amounting to a differentiation of input in terms of the temporal succession of events. Simultaneous synthesis characterizes the visual system, the multi-modality 'body image', orientation of the person in space, and aspects of memory, calculation and language. This function Luria attributes to the parieto-occipital area. Successive synthesis affects both perception and retrieval from memory store of speech and other acoustic sequences, as well as the sequential organization of movement patterns. It is the function of the fronto-temporal area.

Some forms of disordered behaviour are well described in these terms. Breakdown in phonemic analysis and in the perception of rhythmic auditory patterns following left temporal damage is an instance. Again, in visuo-spatial agnosia (Paterson & Zangwill, 1944) it indeed appears that the range of cue utilization is radically restricted, so that the patient must act on the basis of fragmentary and therefore at times misleading information. However, Luria insufficiently examines whether this failure in 'simultaneous synthesis' is genuinely a consequence of impaired integration of individually correctly perceived stimuli or whether the failure of integration is a secondary phenomenon. In some instances, at least of so-called simultaneous agnosia, there is pathological delay in the speed of serial recognition, so that multiple displays are slowly and incompletely identified, whether that identification depends on integration of units, or merely on the recognition of a number of independent items simultaneously exposed (Kinsbourne & Warrington, 1962). The same reservation applies to the elegant studies of deranged exploratory eye movement presented by Luria. It remains an open question whether the recordings represent a primary eye-movement disorder or are secondary to impaired perception. Following Sechenov's insistence that each act of perception has a motor (orientating) component, Luria ignores this distinction, but it remains the case that many perceptual deficits can be clearly demonstrated under conditions of exposure too brief to permit interaction of eye movements. The drawback of adherence to elegant generalization is revealed. It makes for premature closure of inquiry if the findings are merely consistent with the hypothesis.

Luria rarely considers rate of a cognitive process. To him a process is, or is not, available in a given context. Contemporary Western psychology, with its information-processing approach, provides a methodology which could with advantage be applied to neuropsychology. The measurement of rate of processing, rather than concentration on absolute deficits, has the advantage that far less severely affected patients can be used than are usually discussed by Luria, with obvious benefit for the design of valid experimental procedures. On the other hand, Luria's more physiological approach is not negligible. Not all abnormal behaviour can be explained by supposing that the cortex is an elaborate telephone exchange with individual communication channels either active or not (Geschwind, 1965). Pavlovian preoccupation with balance of central excitation and inhibition might well provide a better approach to the notorious variability of impaired performance and to Luria's curious observation that subcutaneously administered caffeine ameliorates certain cognitive deficits which he attributes to 'increase of cerebral tonus'. It might be that damaged brain is inhibited to an extent by intact brain and that this contribution is reduced by caffeine.

As might be expected, Luria is less successful where circumstances do not fit Sechenov's stereotype. He is unilluminating on the Gerstmann syndrome, in which there exists a disorder of differentiation both in space and in time; differentiation between spatially adjacent fingers (finger agnosia) and spatially adjacent numbers (dyscalculia) as well as temporally successive letters in the oral spelling of words. The situation is embarrassing in that, at least if successive synthesis is expanded to include spatial as well as temporal differentiation, the Gerstmann syndrome would be best classified as a disorder of that process, but unfortunately it is a manifestation of posterior parietal disease, well within the 'simultaneous synthesis' area.

Luria is at his most impressive when discussing frontal-lobe function and gives the most coherent available account of the role of this impenetrable territory. His account also fits well with the reported effect of frontal-lobe lesions in monkeys (Rosvold & Mishkin, 1961). The prefrontal lobe guides the strategy of voluntary movement, determines appropriate patterns of exploration and action, and makes movement susceptible to verbal control, both from within and from outside the person. The patient with frontal-lobe deficit is sluggish and inert. If the lesion is orbitally situated, then when he does act, he tends to disinhibited and primitive behaviour. The orienting response is inadequate, both in its autonomic (alpha-blocking, psychogalvanic, etc.) and its motor aspects; exploratory eye movements are ill-organized, so that visual search becomes inefficient. Verbal instruction is heard, correctly repeated, and can release verbal response. But if the command is for the patient to move in some specified manner, it is not obeyed, though it is understood. At the same time, verbal instruction does not, as would be expected, strengthen the orienting response, and block its habituation. Concrete behaviour, as in the interpretation of proverbs, is also described. Perseveration is characteristic, both in simple movements and in thought processes.

Luria's generalization as regards this range of abnormalities is that the frontal lobes normally form programmes for action and compare these programmes with outcomes of the completed acts. The frontal-lobe patients' programmes are contaminated by recurrent action stereotypes, and as the patient can no longer evaluate the outcome of his act, he is unaware of deficiencies and not in a position to correct them.

This striking picture of abnormality appears to be characteristic of severely affected patients, particularly those with large frontal lobe tumours. In such situations motivational factors should have been considered. Both Milner's (1964) account of frontal lobe patients 'breaking the rules' imposed by the examiner during the test situation and the well-known effects of prefrontal leucotomy in tipping behaviour in the direction of psychopathology makes this a point worth persevering with. Luria remarks that the patients do not seem to be responsive to the usual reinforcements. Are they responsive to unusual reinforcements?

In the face of massive lesions it would have been useful to have available documentation of the patient's general cognitive functioning, and one might ask to what extent the observed behaviour is a result of generally lowered cerebral competence due to massive destruction of brain and raised intracranial pressure. How many cases of massive non-frontal cerebral disease behave similarly? While standard intelligence tests have no place in the analysis of cognitive deficit, for which purpose they were neither designed nor standardized, they are invaluable for the demonstration of the extent of normality of the residual unaffected spheres of cognitive function.

Akin to the notion of verbal control of movement is that of the predominantly verbal nature of thinking. Soviet psychologists have been at pains to demonstrate covert verbalization coincident with problem solving, even where the problems are not obviously verbal in nature (Sokolov, 1967), a process which Luria attributes to the left temporal lobe. Verbalization is inferred from electromyographic recordings from the speech musculature. But it is too readily assumed that covert speech activity during cognition is causally connected to the successful outcome of the cognitive act. Zangwill (1964) has reviewed instances in which correct problem-solving was accompanied by wildly inappropriate verbalization. It is crucial to demonstrate that impediments to verbalization downgrades the ongoing performance. Evidence that this occurs when a subject is asked to clamp his tongue between his teeth is presented by Luria, who reports that the effect of this manœvre is enhanced in certain instances of left frontal-lobe disorder. The effect of speech muscle immobility is assumed to be due to a lack of kinaesthetic feedback guidance of inner speech. This is not necessarily so and seems unlikely in view of the very low amplitude electromyographic perturbations that occur during covert verbal processes. Holding tongue and jaws rigid requires tonic innervation of muscle concerned in speech. If covert verbalization relies upon firing of the same neuronal aggregates as those that are active during overt speech, then their prior tonic activity makes it impossible for them to be activated in appropriate patterns, quite irrespective of circumstances in the periphery.

However this might be, this method for inhibiting covert verbalization is so simple that, if replicable, it would provide a powerful test for the notion that while the more verbally recodable the test material, the more will memorizing it become difficult in the presence of left posterior cerebral disease; the converse will be the case with disorder of the right posterior hemisphere (Kimura, 1963). Luria himself does not deal in right–left hemisphere comparisons.

Luria conspicuously ignores negative instances and thus his account under-represents the complexity of the effects of brain lesions on behaviour, and underrates the degree of prevailing ignorance of human brain mechanisms. His work, nevertheless, is the result of keen clinical observation, utilizes promising basic concepts, and constitutes a rich source of hypotheses suitable for and deserving of experimental test.

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PUBLICATIONS REVIEWED

Mental Retardation: Appraisal, Education and Rehabilitation. Edited by ALFRED A. BAUMEISTER. London: University of London Press. 1967. Pp. viii + 419. 105*s*.

The last five years have seen a proliferation of books on various aspects of the study of mental retardation, ranging from critical summaries of experimental research to accounts of methods used in education by individual teachers. This latest contribution is addressed to readers already possessing a general acquaintance with the subject, and is based on the premise that mental retardation is primarily a behaviour problem, whatever biological anomalies may underlie the expression of maladaptation. It follows that, to a large extent, 'treatment' may be seen as any intervention intended to produce a better state of adaptation between the mentally retarded person and his environment, and that this is an area in which the psychologist may be expected to show expertise, both in terms of theory and also in the practical application of such theories. A major part of the book is devoted to discussion of remedial methods, and the approach is frankly stated as both optimistic and empirical. Problems of classification, which for so long have been regarded as the principal concern of psychologists, receive only brief attention in a closely argued chapter on classification by Dr Brison and two excellent contributions by Drs Allen, Jones and Cromwell, one reviewing evidence on perceptual, conceptual and psycholinguistic evaluation and another on the evaluation of personality. The many chapters on remedial methods (covering speech training, psychotherapy and vocational training, among others) include suggestions, which, while they have not been subjected to rigorous experimental scrutiny, are by and large derived from a close examination of the research literature. The editor predicts that this book will not excite the purist, since the policy adopted by contributors is to forgo extensive critical reviews of the literature and instead to emphasize the practical implications of research. Nevertheless, the references quoted are sufficiently numerous and comprehensive to enable the student to go back to the evidence on which most opinions are based.

Two subjects which have not been accorded an exclusive chapter in most other texts are programmed learning and methods of selection and training of institution staff. The former subject, based as it is on a great deal of careful theoretical research, receives rather cursory treatment; if experiments using programmed learning with the mentally retarded are as yet few, perhaps Dr Malpass might have offered a more extensive account of the assumptions underlying this technique, together with a review of some of the more important experimental findings using normal children.

Dr Baumeister's contribution on learning abilities of the mentally retarded constitutes a sober appraisal of research on verbal learning, motor learning and operant conditioning in the mentally subnormal. His conclusion that, compared with normal individuals, mental retardates as a group are inferior in acquisition but comparable in retention is based on extensive evidence. In further concluding that this population is with certain exceptions much less disabled with respect to motor ability than verbal ability he raises an issue which is likely to be the central problem of theoretical and applied research in the next decade, namely to what extent can retarded people, who are characterized by their failure to learn spontaneously, be instructed in the use of conceptual rules. A great deal of time and systematic attention has been devoted to teaching retardates motor and social skills; the possibility of ameliorating their deficit in verbal conceptual areas has as yet hardly been explored, although there are already signs of an increasing awareness of this as a research problem.

The final chapter in the book is a long and exhaustive review of studies on counselling parents of the mentally retarded, a subject too frequently overlooked or scorned by those whose interest is primarily academic, whether their discipline be medicine, education or psychology. In summarizing an extensive literature, and lucidly focusing attention on the problems in this field, Dr Wolfensburger has made a valuable contribution which deserves close attention.

ANN M. CLARKE

The Person in Psychology: Selected Essays. By GORDON W. ALLPORT. Boston: Beacon Press. 1968. Pp. viii + 440. $9·95.

The lack of intellectual cutting edge, the slightly blurred outline, evident in much of Allport's work, and also his great merits—his humane purpose, his respect for work widely different from his own and his comprehensive view of psychology with the human person at its centre—are well represented in these reprinted essays, a selective harvesting which he prepared shortly before his death. It is easy to see why psychologists, who prize above all else conceptual clarity, precise formulation, statements open to experimental test, should find little in Allport to compel their respect; easy, too, to see why they prefer to turn away from a writer who brings to his subject a range of values that makes their discipline seem constricted.

The opening essays on eclecticism in psychology and on six decades of social psychology offer a useful, small-scale survey of the main movements in the psychology of Allport's time. He is doubtful about the progress of the science: 'We do not solve our problems. We grow weary of suggestibility and so investigate persuasibility; personality and culture give way to systems theory; the group mind drifts into organizational theory; rationalization becomes cognitive dissonance; friendship masquerades as interpersonal attractiveness; problem-solving dissolves into programming; pleasure and pain become positive and negative reinforcement; maladjustment becomes alienation; volition gives way to decision-making; no longer does one possess character, one has ego-strength...to me such shifting often seems merely weary and evasive—a device for overlooking previously relevant studies.'

Another group of these papers gives the gist of his work on ethnic prejudice, especially his account of its non-linear relation with religious affiliation, where he demonstrates the importance of distinguishing between 'intrinsic' and 'extrinsic' religious attitudes and usefully complicates the previous simple finding that the religious are more prejudiced than the non-religious. Of several obituary or memorial tributes, one of the most substantial deals suggestively with 'The Productive Paradoxes of William James'. A valuable memorial essay on William Stern draws attention again to a psychologist of great range, whose effort at a comprehensive theory seems inclusive of much that Allport stood for, though Allport understandably stresses points of difference. The papers on mental health and personality development were written for non-technical audiences and the sermon (of a decently secular kind, of course) is never far in the background. It is in fact the rather broad generality of his values that lessens the impact of Allport's work. He says, for instance, of Lewin: 'To him the better life meant always the democratic life, the humane life, the humorous and kindly life.' These are much-handled, well-rubbed counters, and if psychology is to have dealings with values it needs something more sharply incised than this. But at least Allport insisted that human values and psychology were mutually relevant. He stood out against the conception of the subject as a merely factual discipline in which the immature can be satisfactorily taught and examined by the insensitive. He seems at the moment to have fought a rearguard action; but because the problems he dealt with are real and important they will continue to be worked at, whether or not by what the universities choose to know as psychology.

D. W. HARDING

Principles of Autonomic–Somatic Integration: Physiological Basis and Psychological and Clinical Implications. By ERNST GELLHORN. Minneapolis: University of Minnesota Press. 1967. Pp. ix + 318. 80*s*.

For those who still insist, if only for pedagogical reasons, upon a strict compartmentalization of subject-matter into affective processes, cognitive processes, instinct, skill, motivation, etc., Gellhorn's *Autonomic–Somatic Integration* should provide a salutary reminder that, whatever the labels, everything in psychology *is* related to everything else. If it be disputed that this necessarily follows from the existence of a single and unifying physiology, a detailed account, such as this one, of the manifold interactions between the ANS, CNS and endocrine system, leaves no grounds for separatism in psychology.

Ostensibly the book is concerned with the possible, probable and certain roles of the so-called ergotropic and trophotropic systems in a wide, if not exhaustive, range of normal and pathological behaviour.

(According to Hess, ergotropic effects include those associated with sympathetic discharges plus activity of the motor system while the trophotropic syndrome is associated with reduced parasympathetic activity and responsiveness of the somatic nervous system.)

From describing the general characteristics of these systems and the hypothalamic–limbic–reticular 'hub' of their integrative machinery, the book goes on to show their role in various states of consciousness and arousal. In addition to the normal workings of the ergotropic and trophotropic systems, including those which follow from the fact that hypothalamic excitation changes cortical potentials and overt behaviour, consideration is given to deviations from their normal functioning under experimental and pathological conditions.

While the dynamics of the underlying physiology include the reciprocity and breakdown of reciprocity between the two branches of the ANS at all levels, the effect of upward discharges of the hypothalamus upon cortical synchrony and motor outflow, and dissociation between upward and downward discharges of the ergotropic system, the concomitant psychological phenomena range from yoga-like states and paradoxical sleep to conditioning and experimental neurosis. Thus, in connexion with the idea of cortical homeostasis involving corticoreticular negative feedback, Gellhorn discusses recent evidence for the interesting view that the inhibitory mechanisms involved may in fact serve only an antipathophysiological function by preventing hypothalamic imbalance and psychosomatic disease.

All in all, there should be few psychologists concerned with the underlying physiology of their subject-matter who will not find something of interest in the nine chapters of this stimulating *tour de force*. Thus on one page we find 'transference' and other behavioural explanations of therapeutic 'gentling' of neurotic cats replaced by explanation in terms of trophotropic system reactions to cutaneous stimuli, while on another there is an equally down-to-earth discussion of Pliny's succinct observation, that 'omne animal post coitum triste', in terms of a shift in ergotropic–trophotropic balance following the excessive ergotrophic discharges that accompany orgasm. Elsewhere, again reduced to an explanation in terms of these versatile systems and the highly complex interactions of which they are capable, are such apparently disparate phenomena as perceptual defence, displacement activity and the relation between facial expression, emotion and operant learning. On higher forms of learning the author confines himself to the view that those 'including creativity in man would still be linked with the hypothalamic system because even the highest forms of intellectual endeavour are not achieved in an emotional vacuum'.

Perhaps inevitably the main fault with the book is that in trying to cover so much ground some topics as, for example, the relation between emotion and perception, are rather summarily dealt with. The compression of information entailed by integrating within 250 pages the data from over 1000 papers does not always make for easy reading. When the wood emerges from the trees it sometimes appears to be a jungle.

Notwithstanding this criticism, *Autonomic–Somatic Integration*, by an author well known for the fertility of his ideas and considerable synthesizing ability, remains a book of great value to all who are interested in the physiological bases of behaviour.

N. F. DIXON

Cues, Decisions and Diagnoses. By PETER E. NATHAN. New York and London: Academic Press. 1967. Pp. xx+244. 84*s*.

The author discusses three different approaches to diagnosis: to discover aetiology, to describe behaviour pathology and to predict course and treatment. He considers that there is a conspicuous paucity of reliable data on the treatment, aetiology and prognosis of most psychiatric disease entities, and consequently opts for the description of current behavioural pathology. He believes that clinically relevant and reliable diagnostic procedures can and will ultimately be specified and that these procedures should include the decision rules the experienced clinician uses to integrate the important cues for diagnostic purposes.

In pursuit of this aim six of the 12 chapters are taken up with the organization of diagnostic cues into separate linear flow charts and their integration by appropriate sets of decision rules. They attempt to make explicit the implicit diagnostic decision-making behaviour of the experienced clinician. In the reviewer's opinion it is a brilliant and worthwhile attempt. An artist often talks nonsense about his work and it takes a sensitive critic to 'explain' him. Were the artist to try to benefit from having learnt what he had been doing, his products would probably deteriorate. The same fate would be unlikely to assail the clinician who discovered what he had been doing.

An extract from 'Diagnostic Model IV: Abnormal Cognitive Behaviour' may, even without the charts, help the reader to grasp the method.

> (1) If form of patient's thought is disordered (A)/and his thinking appears largely of autistic character (B) (= fundamental schizophrenic symptom)/and he manifests at least one additional fundamental schizophrenic symptom (C)...*diagnose* Schizophrenia; (2) A, B/and no other fundamental schizophrenic symptom...*call* Schizophrenia...; (5) If progression of patient's thought is disordered (F)/and his thinking is characterised by 'flight of ideas' (G)/but logical connections can be apprehended between the scattered thoughts (H)...*call* Manic; (6) F, G/and logical connections cannot be apprehended...differentiate between Manic and Schizophrenic.

Alongside each of these cues are relevant supportive quotations from recognized authorities.

The psychosis *v.* neurosis model leads off with five symptoms pathognomic of psychosis. It is only in the absence of these that certain symptoms and signs lead to a diagnosis of neurosis. This vital point might profitably have been emphasized more. Even psychotic depression cannot be diagnosed until the symptoms and signs pathognomic of schizophrenia have been excluded.

There seems little doubt that adherence to some such systematic decision-making would increase the reliability of psychiatric diagnosis. It is greatly to be hoped that this challenge will be widely accepted by those who share Nathan's belief that reliable diagnosis based on current behavioural pathology is an essential precursor to aetiological diagnosis. In justifying his emphasis on current behavioural pathology, Nathan omits perhaps the most potent argument. If we utilize both personal history and present or recent symptoms and signs for arriving at a single diagnosis, we are prejudging issues which still require investigation. Let us suppose that, on the basis of present symptomatology, we are somewhat undecided between two diagnoses x and y, but prefer y. The personal history suggests that 3 yr. earlier the patient may have suffered from an untreated x. This may tip the balance in favour of a present diagnosis of x. The patient is readmitted some years later. The then present symptomatology and the personal history both now become part of the personal history. It is noted that the patient was thought to have suffered from x. This may or may not influence a decision on the basis of readmission symptomatology away from y to x. All that we may be assessing is the degree of suggestibility of the clinician. Had the diagnosis both 3 yr. earlier and today been based solely on present symptomatology, these data would have contributed to the question as to how often patients continue to present with similar symptomatology. If we wish to study the history of different psychiatric syndromes, we must exclude history.

The review chapter on the reliability of psychiatric diagnosis is rather inadequate and omits mention of those who might be regarded as the author's supporters, such as Kreitman, Norris, Schmidt & Fonda and Foulds, who seems to be allocated to the wrong camp. Despite this affront, the reviewer is obliged to conclude that this book sets out to do something new and does it well.

G. A. FOULDS

Childbearing: its Social and Psychological Aspects. Edited by STEPHEN A. RICHARDSON and ALAN F. GUTTMACHER. New York: Williams & Wilkins. 1967. Pp. xvii + 334. 68*s*.

In a small suburban stationer's recently, I counted among the paperbacks no fewer than four books on the process of childbirth itself, quite apart from the half-dozen on baby and child care. Perhaps what is surprising is not that childbirth now arouses such interest, but that we have taken so long to make information freely available on what is, after all, the major creative event in most women's lives, and, for both men and women, the most rapidly productive of change in life-style. Nevetheless, the content of these four paperbacks was almost entirely medical; none showed the least consciousness of the wide differences in social background, values and beliefs, or immediate material circumstances which exist even within the limits of the community served by one stationer's shop.

There is, however, no lack of research by psychologists, sociologists, anthropologists and the medical profession itself which is directly relevant to the social and psychological context of childbirth: if only one knows where to find it. Often valuable information is buried in more general studies of childhood, parenthood or individual ethnic groups. This book in fact grew out of a perceived need for a systematic review of such studies: its original sponsors (the National Institute of Neurological Diseases and Blindness) are medical, and its aims are to promote a better understanding of the psychological and social factors that contribute to medical risk in childbearing women and their offspring.

The first three papers and the last provide a most useful critical review of relevant research, covering both humans (mainly English-speaking) and infra-human mammals. With the exception of Dr Illsley, the authors present no particularly original ideas, but they cover the ground very efficiently; there is some repetitiveness in the earlier chapters, and one could wish for a firmer editorial hand. To this reviewer, however, the long paper on cultural patterning by Niles Newton, introduced, concluded, and apparently both inspired and supervised by Margaret Mead, is the corner-stone of the book, giving form and perspective to the rest. Dr Mead (I am informed) is out of fashion nowadays among academic anthropologists: their loss, for how exactly she puts her finger on the essential point, and what value for money she always gives in terms of insights of real importance! She and her collaborator transform a solidly useful work of reference into an exciting book. ELIZABETH NEWSON

Assessment of Human Characteristics. By E. LOWELL KELLY. Belmont, Calif.: Brooks/Cole. 1967. Pp. 114. 16*s*. 4*d*.

This section of the 'Basic Concepts in Psychology Series' attempts, as the author states in his brief introduction, 'to provide the reader with a broad overview of the approaches to, methods of, and complex problems of human assessment'. The small volume is beautifully produced. The print is clear, the style lucid, the contents well marshalled and the whole delightfully free from unnecessary waffle.

The first half of the book is mainly factual. Thus, chapter 1 covers the reasons for assessment, chapter 2 the aspects of human beings that can be assessed (body-build, skills, traits, attitudes, interests, drives, etc.), and chapter 3 the methods of assessment in current use (life-records, questionnaires, tests, rating scales), while in chapter 4 the author discusses questions of reliability and validity.

In the next half of the book the author gets into rather deeper water and starts to analyse the assessor himself, finishing up with some interesting speculations about future trends.

As a general introduction to the subject it is hard to imagine how the first part of this book could be bettered; but having described so persuasively the importance of personality assessment and how it should be carried out, the naive reader may feel a little confused at the author's own apparent doubts about the value of existing techniques. Professor Lowell Kelly's suggestions about how these could be improved upon would certainly appeal to the sophisticated, but in a book written primarily for the beginner, an essay on this subject seems slightly out of place. Perhaps the author feels that novices should not be lulled by too much science into a false sense of security: and in this he certainly has the present reviewer's sympathy. MOYRA WILLIAMS

Communication and Comprehension. By J. M. TRENAMAN. (Edited by E. M. HUTCHINSON.) London: Longmans, Green. 1967. Pp. xx + 212. 35*s*.

When reading bibliographies one is always struck by the number of theses which are unpublished; this book is one which got away. In spite of the escape and Mr Hutchinson's careful editing and reorganization, the book reads as a thesis. The limitations of the book are manifest in the title which the author explores in depth. The first part of the book is concerned with the way in which a large number of individuals answered comprehension, attitude and previous knowledge tests of some seven programmes, each presented in comparable visual (television), auditory (radio) and reading form to matched groups. He found that for a group of programmes which varied very widely in content the differences between the three formats were negligible and not replicated when compared with the differences arising from other causes.

The second stage of the work was to select 64 programmes containing permutations of seven factors deemed important within a programme and to expose them to viewers and to assess comprehension. The factors chosen were interest, verbal difficulty, verbalization rate, number of points, concreteness–abstractness, personification and visual movement. By analysis of variance only concreteness–abstractness significantly affected comprehension, whilst number of points made (whether greater or less than 11) and personification also seemed to influence comprehension. Psychologists have long had a knack of proving the obvious and Trenaman, using the one score on a comprehension test involving both multiple choice and sentence completion (questions which were supposed to assess general principles involved, major and minor points, in a total score of 18 marks) could hardly have proved otherwise. The reviewer feels the exercise could have been more worth while with smaller samples and far more exacting tests (not only of comprehension) which could allow variables such as visual movement and personification greater scope.

Trenaman also endeavours at considerable length, and unfortunately in the empirical rather than the theoretical tradition, to map out statistically differences both between programmes and between programme recipients. Here the data read tediously as there is no theme, but much useful material is stored here for the patient numerical reader, although one doubts if television producers will read this section. However, the thoroughness of his work truly deserves admiration, what is set out to achieve is achieved and by the most rigorous techniques. Why though did he not apply statistics which attempt to match sets of variables on either side of the equation, such as canonical analysis?

One cannot help but hope that Trenaman's finding that dramatization of serious material is the most effective method of communication to occupational groups 4, 5 and 6, is responsible for programmes like 'Cathy Come Home'; but one cannot help feeling that such programmes are rather due to the sheer innovating power and commitment of their young producers.

GRANT NOBLE

Behavior–Genetic Analysis. Edited by JERRY HIRSCH. New York and London: McGraw-Hill. 1967. Pp. xvii + 522. 100*s*. 6*d*.

Behavior–Genetic Analysis grew out of a conference held at Stanford University during the summers of 1961 and 1962. The editor contributes preface, introduction and epilogue to its four main sections and over 80 pages are devoted to an excellent bibliography and index.

The problem of the evolution of behaviour is examined in the opening section from two widely different viewpoints: the genetic and the phylogenetic. Following an introduction by Caspari defining these two approaches, two chapters by Washburn & Shirek and by Rothenbuhler discuss the phylogenetic argument in relation to the behaviour of man and social insects respectively, and two chapters by King & Manning discuss the genetic mechanisms involved in the response to selection and in evolutionary changes.

The second section, 'Genes and Mechanisms', opens with a review by Caspari of current genetical opinions on gene structures and function and the role of gene action in development, learning and memory. Three chapters, by Ginsberg, by Hamburg and by Hsia, examine the available evidence on the action of genes having behavioural effects. This evidence is largely confined to behavioural differences controlled by one or a few genes of major effect, and amenable to classical, Mendelian analytical techniques at the genetical, physiological and biochemical levels. It is not surprising, therefore, that in these relatively simple situations the analyses reported have proceeded far beyond anything so far achieved with behavioural differences controlled by polygenic systems in which large numbers of genes are involved. Unfortunately, most behavioural differences are of the latter kind, thus presenting the greatest challenge to the investigator and the greatest incentive for successful analysis. It is also the case that progress with the analysis of such polygenically controlled differences is limited by the failure of psychologists to appreciate the genetical problems thus posed at the levels of experimental design, analysis and interpretation.

The third section of this book is therefore particularly important because it attempts to clarify these problems and to present alternative examples of the best methods currently available for investigating polygenically controlled characteristics. The chapter by Roberts achieves this aim by examining the approaches applicable to randomly mating populations in a clear and precise way, but in so doing he confines his discussion to one of its lesser used experimental approaches. Hirsch achieves an even more restricted aim of describing the technique of chromosome assay, which he successfully imported from genetics into psychogenetics, but which at the present time has no relevance to organisms other than *Drosophila* (fruit flies). Bruell's chapter, in contrast, achieves neither clarity of presentation nor genetical respectability. Under the guise of a genetical investigation of the relation between the structure of a genotype and the selective forces operating on behavioural differences in the wild, a relation well-established for non-behavioural characters, he merely presents his observations from diallel sets of crosses between inbred strains of mice, alternately in numerical, verbal and graphical form. None of them are particularly illuminating, still less analytical or informative. The following chapter by Broadhurst, based on the biometrical analysis of the same diallel crossing programme, yields a simple and comprehensive account of the analyses, models and interpretations, illustrated by his own data from rats. Furthermore, this paper is unique among those in this section in discussing the problems of experimental design and scaling in relation to the subsequent analyses and in stressing the particular need for independent replication to provide correct estimates of the error variation—a need which Bruell, in common with many other psychologists, has neither understood nor allowed for in his investigations.

The fourth section concerns itself with conceptual and methodological problems peculiar to behaviour genetics. In two chapters McClearn and DeFries underline the importance of individual differences and their adverse effect on attempts to devise laws of general applicability. This, in fact, is a conclusion that emerges from the results presented in the third section. Roberts, on the other hand, is more concerned with the broadening impact that behaviour genetics is beginning to have and will continue to have on genetics itself. A stimulating paper by Thompson draws attention to the need to understand how behaviour works and underlines the danger of stopping short of a full understanding of both its genetical contact and the interaction between genes and the environment. Certainly, none of these cautionary comments strike a geneticist as novel and indeed they have become much less novel to workers in behaviour genetics in the half decade this book has been a-publishing. The final paper by Spuhler & Lindzey on racial differences in man has little to offer on differences that are not major genic in origin.

It is a pity, in view of both the promise and the solid achievements described in this volume, that the editor's comments in the lengthy epilogue are carping, over-critical and largely negative. These are the jottings of a Jeremiah. After reading the epilogue one wonders how genetics itself, either as a name or as a science, has survived so long, let alone flourished so successfully. Hirsch allows himself to be carried away as frequently by words as he is by numbers. He 'explains' the so-called 'Tryon effect' in terms which constitute a poor memorial to the pioneer to whom the book is rightly dedicated in the year of his death, for they rule out any investigation of genetical segregation for quantitative characteristics controlled by many genes and ignore established explanations of this kind of effect which abound in the literature of biometrical genetics. In discussing the procedures of biometrical genetics, including diallel analyses, he exposes his lack of understanding or sampling theory and 'fixed sample' analysis. But some of his criticisms—for example, his comments on heritability and genotype–environment interactions—while exaggerated and addressed to the converted, nonetheless have a general validity, as a careful reading of some of the chapters in the third and fourth sections of the book shows. But it is surely a sour note on which to end an excellent book, which all interested in the future of psychology and of genetics will do well to read, and those of us concerned especially with the hybridization of both cannot afford to miss.

J. L. JINKS

Influences on Parent Behaviour. By LOIS M. STOLZ. London: Tavistock. (Stanford Studies in Psychology.) 1967. Pp. ix+355. 65*s*.

The author is Emeritus Professor of Psychology at Stanford University. The major antecedents to the research on which this volume is based were the research projects of the author and collaborators in Palo Alto, California, and of R. R. Sears and company in Massachusetts which resulted in *Patterns of Child Rearing*.

The investigation of children's behaviour and development by systematic observation and experimental control is a long-established tradition in psychology. Later came the study of parental practices, in advanced and in primitive societies, in order to identify and hypothesize these as determinants of the observed variety of childhood behaviour and individual development. The present work takes the inquiry back a further stage; it is an investigation into the 'pertinent values, beliefs and other influences leading to' parental practice. It appears to be the first large study of what parents think they are doing, what they aim to achieve and of the origin of the influences which have shaped their relationship with their offspring. Therefore its findings are of the greatest interest, not only to a developmental psychologist, but also the social psychologist whose focus of interest may be the communication and reference systems which impinge on parents in a technically advanced society.

The informants were both parents of 39 families living in a residential area south of San Francisco. In size of family, type of residence, age of parents, educational background, religious affiliation, ethnic origin and social status the sample represents a wide range, but with some bias towards rising social mobility and the higher educational and occupational groups. In terms of these social and familial variables it is a small group of subjects, but the volume of material for analysis was very great and the reliability of the findings should be exceptionally high for data obtained by interviews. An average of four 1 hr. interviews, conducted individually with each parent, were tape-recorded and the typed transcripts were coded and analysed.

The interviewees' statements about their concerns and their perceptions of their roles as parents were broadly categorized under the following headings. 'Values' include the provision of education and emotional security, the exercise of control, economic support and parents' aspirations for their children, personally and socially. 'Instrumental beliefs' cover references made in the interviews to convictions that particular practices have particular consequences, notably biological and moral outcomes. 'Descriptive beliefs' include processes of learning, the effects of reward and punishment, the relative influences of heredity and environment, and what boys and girls are normally like at different ages. 'Other influences' are subcategorized into the composition of the family, the behavioural setting, the previous experiences of the parents and sources of communication. This last heading includes interpersonal (such as teachers and doctors) organized groups, and the mass media.

Nearly 100 tables present very clearly such data as the relative frequencies with which topics were raised in the interviews, comparisons between mothers and fathers, intercorrelations within systems of values, beliefs and influences, and correlations of these elements with demographic and other variables. The text is illustrated by verbatim quotations from the interviews, which read with great authenticity.

The method employed in this research has its own intrinsic limitations. Some critics might draw attention to the unconscious forces that direct a parent's relationship with children. At the other end of the introspection scale are the things left unsaid because 'it is...possible that parents take their beliefs about some...goals for granted and therefore do not think of them as influences on their behaviour...For many parents, the influence of parental example may be so much an accepted fact that they failed to mention it.' However, there is no doubt that this important work is a rich mine of information and hypotheses concerning the influences that operate in a very complex area of human behaviour.

C. J. PHILLIPS

Verbal Behavior and General Behavior Theory. Edited by T. R. Dixon and D. L. Horton. London: Prentice-Hall International. 1968. Pp. 605. 93*s*.

This book consists of papers given at a conference arranged by the editors with the expressed intention of reducing the discrepancy between the large amount of data which has appeared from verbal behaviour laboratories and the small amount of theoretical integration of it. S–R conceptions were to serve as the common point of reference. The aim was not achieved and the discussions centred on the general inadequacy of S–R theory (whatever that may mean) with reference to verbal behaviour. The book is divided into five sections. In the first, Tulving considers some effects of associative variables on free recall; Pollio discusses the development of techniques for studying associative structure and Kjeldergaard summarizes in an odd mixture of empirical generalizations and Hullian-type assumptions the principles thought to underlie transfer and mediation processes. Deese and Mandler comment on these papers favouring Tulving's explanation of increasing recall efficiency over repetitions by an improved organization of the S (subjective) units into which the E (experimenter-presented) units are reclassified. The associative structures investigated by Pollio are seen as sources of facilitation with theoretical developments appearing as the hierarchical organization of the subject-imposed structure is elucidated (Mandler). Kjeldergaard's paper makes clear why S–R analyses are out of favour. This attempt to look at certain characteristics of words and their interrelationships within some hypothesized storage/retrieval system is not continued in the second section. Within the overall framework of S–R theory, Young looks critically at serial-learning studies, particularly at the problem of the functional stimulus in serial learning. He concludes that simpler techniques, specifically paired-associate learning, may be more useful in the analysis of verbal behaviour in general; the study of serial learning phenomena is of interest but only to the individual interested in serial learning. Battig, disagreeing with the view that paired-associative learning is a simple form of learning, presents a critical account of its rationale and techniques. Simplification is claimed to have made the application of S–R laws to an analysis of paired-associative learning defensible but inadequate. This inadequacy, however, has produced neither the recognition that S–R concepts are too simple for such an analysis, nor that paired-associative learning is a complex phenomenon. Useful theoretical replacements are not suggested. Keppel provides a very thorough review of retroactive and proactive inhibition, which is firmly in the Northwestern/Berkeley tradition, and finally seems to support the limited reference view of Young when he claims that the main theoretical and empirical support for interference theory will continue to derive from the RI/PI model of the forgetting process. A brief critical paper by Asch emphasizes the restrictions placed by the empiricism associated with the S–R analysis on the development of constructs about underlying processes—association by contiguity is deemed to comprise a restricted segment of the area of human learning—investigations and analyses of structure, construction and innovation are required.

The third section is on processes related to verbal learning but not to each other. Bourne takes a critical look at S–R analyses of concept learning, Kanfer reviews the current status of verbal conditioning and Maltzman discusses theoretical concepts of semantic conditioning. Dulany, in an extended paper on awareness, rules and propositional control, and Kendler take on the job of evaluation. Section 4 is on psycholinguistics; in it McNeill proposes a nativist approach to syntactic aspects of language acquisition and Garrett & Fodor summarize some linguistic constructs of the transformational type and analyse relevant investigations of performance. It is claimed in both papers that S–R theory is unable to meet the requirements of linguistic theory on either competence or performance criteria. Johnson analyses sequential verbal behaviour investigations and finds that the data compel a recognition of structural rather than simple associative relations in the reproduction of verbal sequences. Bever and Osgood provide interesting and critical papers. Bever supports the views that S–R analyses are inadequate, but raises a number of relevant issues; for example, the intuitive grounds of grammatical judgements, the relation of performance measures to competence, and of associative relations to deep structure. Osgood proposes an S–R representational mediation approach to the same problems.

Discursive papers by Cofer, Jenkins, Postman and the editors complete the book. Cofer is intrigued by the notion of associative relations in deep structure (to paraphrase Deese, it is usual to have something to say before it is uttered), Jenkins is impressed by the linguists and the problems which they raise for S–R analyses, and Postman defends the current analytical approach. The editors conclude with the view that S–R theory has justly been criticized.

This is a lengthy book with a fair amount of verbiage which might have been pruned, but far more which is of clear current interest. The limited scope of traditional studies of SL and PAL and the paucity of S–R formulations of verbal behaviour are demonstrated. More complex models of the underlying organization are hinted at—although it is depressing to see how little progress has actively been made beyond the 'chunk'. Transformational notions of grammar are currently issues of discussion and investigation but these rules are remote from the more central issue of the organization of the material at the deep-structure level, around which investigations of learning, memory and concept formation are often centred, and on which the syntactic forms are based. The gaps are clear—so is the lack of progress in filling them with the units, the organization and the rules of interaction of the necessary theoretical structures.

W. A. MATTHEWS

The Discrimination Process and Development. By BRIAN J. FELLOWS. (International Series of Monographs in Experimental Psychology, vol. 5.) Oxford: Pergamon Press. 1968. Pp. 228. 75*s*.

At a recent conference on road safety, an H.M.I. reported that children have been doing their kerb drill perfectly correctly: then walking directly in front of the oncoming traffic. The fatuity of concentrating the child's attention on the motor movements associated with looking, and the activity of stepping off the kerb, without ensuring that he also learns to judge whether or not the road is clear, seems obvious enough once it is stated. Yet much current psychological theorizing appears to lead in precisely this direction.

The basic question with which Dr Fellows is concerned is how to train children in discriminating between two or more objects which seem to have functional equivalence for them. Quite apart from its practical applications, the question raises some profound theoretical issues. One is bound to be intrigued by a monograph which proposes to put the problem of discrimination into some kind of developmental perspective; perhaps, in the present state of knowledge, one is equally bound to be disappointed. Dr Fellows presents us with 190 pages of theoretical discussion, neurologizing and model-building, in the course of which he provides a well-documented summary of many divergent points of view. He also includes one or two detailed summaries of important papers which are not easily accessible elsewhere. At the end of it all, however, nothing very new emerges.

The reason for this unsatisfactory outcome probably lies in the inadequacy of the conceptual tools used to describe the problem. Although Dr Fellows admits the inadequacy of the traditional S–R approach, he in fact adopts essentially the same approach himself, albeit disguised as an input–output model for information processing. From the outset, in fact, the stimulus is conceptualized as an entity which exists to be fed into the organism from without; and, from this point on, the organism is inevitably treated as no more than a switchboard device for connecting discrete inputs (stimuli) with discrete outputs (responses). Given such an orientation, the outcome is also a foregone conclusion.

Basically there are two possible reasons why stimuli are not discriminated: either the correct response has not been successfully attached to the correct stimulus, or the stimulus has not been correctly registered. With regard to the first alternative the solution is to attach some mediating response to the stimulus, and at this point we have the usual reluctant admission that mediating responses may be internalized and hence not observable. To resolve the second possibility, it may be necessary to get the organism to 'orientate' the stimulus by looking in the right direction, or by making the correct pattern of eye movements. Once again, however, it has to be conceded that this may not offer a sufficient explanation in all cases, and this leads to the notion of implicit, or internalized, orientating responses.

However, the book is by no means without interest. There is much incidentally useful discussion of the kinds of discrimination which children seem to find particularly difficult and the strategies to which they regress when this happens. There is also some description of the *ad hoc* methods which may be used to train a discrimination in practice; and, although it is difficult to relate these procedures to any known theory, they do at least suggest a possible new line of empirical research.

JOHN NEWSON

The Therapeutic Relationship and its Impact: a Study of Psychotherapy with Schizophrenics. Edited by CARL ROGERS. Madison: University of Wisconsin Press. 1967. Pp. xix + 625. 93*s.*

Carl Rogers and a group of 15 psychotherapists, working in Mendota State Hospital with a total team of over 200—which included psychologists, nursing staff, independent psychiatric judges, independent 'raters' of results and technicians—has produced what is perhaps the first extensive and truly scientific study of the nature, possibility and results of psychotherapy. Previous attempts at validation or non-validation of psychotherapy, such as Eysenck's statistical studies, concentrated on results, and often seemed motivated by a desire to prove that psychotherapy produced no results. The implication that, in that case, personal relationship has no importance in human life, was passed over. To study the subtle process of psychotherapy by such means was like trying to measure millimetres with a yardstick, which could result in concluding that there are no millimetres.

Rogers' large team, working over 5 yr. and organized from the University of Wisconsin Psychiatric Institute, did not make the mistake of studying simply gross overall results, but investigated something much more important: the actual nature of the psychotherapeutic relationship, and of the small, cumulative, subtle changes that occur in it, in both the patient and the psychotherapist; and what kind of relationship best promoted changes in the patient in the direction of mental health, cure, maturity or what not, i.e. the process of psychotherapeutic change. The patients were all hospitalized schizophrenics, and it was found that while the less ill psychoneurotics concentrated on self-exploration, the more ill psychotics were all concerned most with relationship formation. This finding confirms the general direction of development in psychodynamic theory over the last 25 yr. or more, which has been to recognize that 'object-relations' (i.e. ego-growth in personal relationships) are the fundamental psychodynamic problem. Rogers found that, at the end of the study, the theory with which they started had been supported by the evidence, namely that 'it was the patient's perception of the therapist's attitudes which was most crucial in predicting therapeutic movement and therapeutic outcome'. This finding encourages those of us who can only be lone workers, without the benefit of team co-operation and with 'time' always under pressure from too many patients for research to be very detailed, to feel we are on the right lines in stressing ever more and more this very factor of the 'therapeutic relationship' as fundamental. It is not a question of 'cured' or 'not cured'. The nature of human living does not permit of such final judgements. It is a question of what kind and degree of change have gone on in this particular relationship which was aiming at psychotherapy.

Those under therapy were matched with a control group not under therapy but otherwise receiving the normal hospital psychiatric treatment. No great claims were made, but rather the effort was directed towards accuracy of assessing what actually happened. Considering that the patients were chronic schizophrenics, who could not be regarded *a priori* as promising, the actual results were encouraging; small changes but of the right kind and in the right direction, and a subtle strengthening of the basic personality. The patients had 2 hr. of therapy a week, for 4 months to 2½ yr., according to their period of hospitalization. At the end the therapy group showed 'a slightly better rate of release from hospital, and this differential was maintained a year after termination of therapy. The therapy group also showed a number of positive personality changes which were not evidenced by the control group.' The differences, it was noted, were not great, but, with such chronic patients, were enough to be meaningful. Thus, 'At the conclusion of the sampling interviews it would be accurate to say that the group showing the greatest openness to experience, the greatest spontaneity, the greatest capacity for communicating themselves, was composed of those who had been exposed to the highest level of therapeutic conditions.' Moreover, 'attitudes of the therapist were perceived by the patient as the *only* agent of constructive personality change'.

It must be left to the interested reader to follow the long and detailed account given of the methods, tests, checks and counter-checks, and the high level of objectivity maintained in this study of the subjective processes of human experience. For those who want to know what psychotherapy is about, this book is of great importance.

HARRY B. GUNTRIP

Psychological Measurement and Prediction. By PAUL HORST. Belmont, Calif: Wadsworth. 1966. Pp. xii+455. 72*s.*

The following chapter headings would indicate that here is the familiar stuff about psychological tests: measurement in psychology, test domains, types of tests, conditions of administration, quantification and interpretation of test results, covariation of measures, multiple measures, the combination of measures, factor analysis, characteristics of tests, test item statistics, test statistics as functions of item statistics, maximum value statistics, effects of chance on test statistics, test homogeneity, test score and error, reliability, parallel forms, statistics of altered length tests, test specificity, validity, multiple prediction, item analysis, speed *v.* power tests, effects of sample selection, and age and test performance.

The many available texts differ not so much in content as in style of presentation. Professor Horst certainly has a distinctive style. Most noticeable—in a perceptual sense—is that the many 'figures' contain only calculations and numerical data. What Thurstone said of Burt in *Multiple Factor Analysis* also applies with equal force to Horst: 'he does not have a single diagram in his text. Perhaps this is indicative of individual differences in imagery types which lead to differences in methods and interpretation among scientists.' The algebraic style is also distinctive. Familiar equations and formulae are generated under the control of explicit definitions and precisely stated goals. Both matrix and scalar algebra are used, where others have only used the latter. Further, numerical examples are for Horst merely the substitution of numbers for symbols rather than truly pedagogic or conceptual devices. The style also carried over to verbal expression. For example, in Horst's language the calculation of difficulty levels 'simply involves a determination of the vector means for the item variables in a matrix of item scores' (p. 385). This is true but pedantic, one is tempted to say; but it is obvious that Horst is not making a special effort to say it this way.

The psychological content is extremely simple; so much so that it approaches the naive in places. Horst does not waste words on doubtful psychological subtleties. The truth is that he cares little for theory in any accepted sense. He is 'inclined to be critical of some of the more traditional reliability and validity theories' (p. v) and he is therefore sparing with historical references. He specifically excludes Lord's extensions of true score theory, and there is not a word about the more recent 'generalizability theory' of Cronbach *et al.* In common with many books about tests, that part of statistical theory which concerns the distinction between population and sample is avoided, but more forcibly than usual: 'The population is of course a theoretical construct, and as such is subject to all of the methodological and epistemological criticisms of theoretical constructs in general' (p. 66). So much for theory and test theory in particular.

Of course, Horst does not claim to be eclectic, nor to have replaced traditional concepts 'with completely adequate models' (p. v). The challenge is to find *any* model in this book, psychological or otherwise. Here is a useful collection of crisp algebraic proofs, but the psychological content is drab. Perhaps this is a true reflexion of the state of the art.

PHILIP LEVY

Perceptual Development in Children. Edited by ALINE J. KIDD and JEANNE L. RIVOIRE. London: University of London Press. 1967. Pp. 550. 100*s*.

This collection comprises 17 articles grouped into six sections: one each about physiological, social, affective and cognitive factors of perception, a review of theories of perception and a nuclear set of four chapters about the development of vision and audition. The editors have apparently allowed individual authors a freedom to choose their own definitions of perception and child psychology resulting in a diversity of articles, some primarily concerned with thinking and learning, and others which do not mention children. Ellman's interesting and persuasive pursuit of the biochemistry of synapses successfully evades the book's title on both counts. Pronko's not so critical review of theories of perception has no special relevance to children, and although McCarthy keeps the focus upon children she comments interestingly on almost all areas of language development other than the perception of speech and language.

The workmanlike reviews of the child's developing perception of colour space and movement in vision, and pitch, volume and timbre in audition are clearly embraced by the title. Fantz's chapter on the pattern recognition is particularly crisp, and while Lovell's review of the child's scientific thinking might not be considered mainstream perception, it is good child psychology. Several specific research reports (Flavell, Ames, Goldstone & Goldfarb, Kagan) give a firm empirical anchor to a book in which some chapters show aspirations to metaphysics: Garai would find it difficult to specify empirical tests of his psychoanalytically based notions about sex typing, and Frank's chapter on the development of philosophical and moral concepts reads like a disjointed and weak sermon.

One worthwhile exercise when reading this book is to try to account for the variability in the quality of the contributions. One suggestion is that it can be attributed to the type of theory underlying research, particularly the extent to which the concepts mentioned are anchored in empirical investigations. The chapters which stay close to what children have been observed to say or do are well organized and easy to understand and evaluate. Those which move towards grand theory confuse classification and explanation, description and prescription: it is these chapters which end with mutterings about a lot more work needed, but omit any specification of just what or why, whereas the empirically based chapters can usually pinpoint the information required. The impression is that careful observation and experimentation of limited areas lead to useful research and eventual theoretical advance, but general speculation remains detached and barren.

PETER ROBINSON

Cleft Palate and Communication. Edited by D. C. Spriestersbach and Dorothy Sherman. New York and London: Academic Press. 1968. Pp. xv + 291. 112*s*.

The stated aim of the book under review is 'to present a systematic review devoted solely to the work which has been done to date on the communication problems of individuals with cleft lip and palate'. The necessity for such a book can be seen as a consequence of the recent upsurge of research into the area by specialists in many different (but related) disciplines. The emergence of journals specifically devoted to the problem (*Cleft Palate Bulletin* and the more recent *Cleft Palate Journal*) has simplified information retrieval but clearly cannot fulfil the need for an easily obtainable source which collates work from the preceding 30 years. The editors have seen their task as promoting such an organizing statement. They have produced a volume which reviews what has been done, assesses trends and identifies gaps in knowledge so that future work can be planned in a maximally useful way.

The eight chapters, each by experienced specialists, are as follows: (1) Anatomical and Physiological Bases for Speech (Huntingdon). (2) Acoustics of Speech Production and Nasalization (Curtis). (3) Speech Characteristics of Individuals with Cleft Lip and Palate (Moll). (4) Etiological Bases for Speech Problems (Morris). (5) Audiological and Otological Considerations (Prather & Kos). (6) Psychosocial Aspects of Cleft Palate (Goodstein). (7) Diagnosis and Therapy (Shelton, Hahn, Morris). (8) Some Professional Implications (Spriestersbach).

The individual chapters are of variable quality and assume different levels of sophistication and motivation in the reader. Chapter 1 is rather elementary and makes little effort towards theoretical integration. Chapter 2, although it has a paucity of references, does present data on the acoustics of speech production in a systematic fashion. Whilst dealing mainly with normals, interesting implications for cleft-palate speech are drawn. The third chapter is most rewarding and enlightening. A coherent, evaluative review of a large amount of literature is given. The article organizes the vast subject-matter extremely clearly, with frequent summaries, lucid tables and sensible definitions of terms. What emerges from this chapter is a clear picture of the speech characteristics of the population; an impartial assessment of the stage and status of work in the field and clear directions for future studies. The next chapter, on aetiological bases for speech problems, is a logical successor. Given the speech characteristics, the author attempts to identify those factors that perpetuate or precipitate the communication abnormality. It is interesting that what emerges is an enumeration of factors not unlike those commonly identified as being significant in articulation and voice problems of individuals without clefts (e.g. amount of stimulation, hearing acuity, dentition, etc.). Chapter 5 is important because it makes quite clear that the otologist has an essential place in the team treating any cleft-palate individual. The incidence and type of hearing loss and their implications in this population are intelligently and concisely presented. The only chapter to treat the subject from a developmental viewpoint is chapter 6. In addition to looking at psychosocial problems from the child's own viewpoint, the chapter includes studies of the effects of the family group's attitudes and expectations. The studies are then placed within the wider context of socio-economic and cultural factors. The chapter concludes with certain very practical guides and directives for future research. Chapter 7 (on diagnosis and therapy) is disappointing. It contains much useful information, but is presented in an unsystematic fashion with general therapeutic principles scattered amongst special drills and contradictory experimental findings.

Whilst the scope of the book is fairly wide, it is apparent that there are certain areas conspicuously absent. No consideration is given to studies of the incidence of the defect. More importantly, the aetiological significance of genetic, biochemical and nutritional factors, and those relating to age of mother, number of pregnancies, etc. is not discussed. The areas of orthodontics and prosthetics have been totally ignored. It would also have been useful to have a chapter on surgical aspects, especially as it is well known that in recent years there has been a radical change in techniques and principles, accompanied by an increasing awareness of their implications for speech. If the book is to be used by a wide variety of workers interested in the problem, then the topics mentioned above are serious omissions. If, furthermore, as the editors claim, the book is intended for a sophisticated audience, it seems pointless to have included elementary chapters on anatomy, physiology, acoustics and professional ethics, to the exclusion of these topics.

The general weakness of the book is in treating the subject population as homogeneous. It is clear that the population must be subdivided in terms of age of individual, type of cleft, type and time of treatment (surgical and/or prosthetic). In addition, recognition must be given to the fact that function is not truly predictable from anatomy. A dynamic approach is necessary, for both the child and the problem change; this should be reflected in all research and inherent in all approaches to the problem.

CHARLOTTE V. GAMSU
JOHN C. MARSHALL

Science, Philosophy and ESP. By CHARLES MCCREERY. London: Faber & Faber. 1967. Pp. 199. 32*s.* 6*d.*

This book begins with a consideration of particular cases of supposedly 'panarormal' happenings—the McConnel affair, Soal's results with Basil Shackleton, Lord Charles Hope's work with Rudi Schneider, and the less well-known case of Eusapia Palladino, supposedly capable of 'psychokinesis'. Next comes a philosophical discussion about problems of repeatability in the case of ESP experiments. In part 2 the author puts forward his own theory, namely that for successful ESP the subject must be 'unconflicted', i.e. free from feelings such as embarrassment or frustrated emotion. On this basis he makes certain predictions about the EEG records and muscle tone of subjects while they are being successful at ESP; for example, that there will be accelerated alpha rhythm. 'The observation that when ESP occurs the subject's alpha frequency is accelerated will be *repeatable*...One would have achieved a *generalization*—something not hitherto achieved in parapsychology' (p. 167).

The methodological discussion in this book, particularly over the issue of repeatability, seems to me good. Moreover, there is a commendable absence of diatribe. In particular, McCreery is correct, in my opinion, in avoiding controversial labels such as 'materialism' which continually figure in discussions of ESP. Commendably, too, he does not confront his readers with the bogus dilemma: '*Either* admit to belief in ESP *or* offer your own explanation of Shackleton *et al.*'; the dilemma is bogus because one can simply reply: 'If you want me to take ESP seriously, *you* should be telling *me*; and if you can show me that the Shackleton findings, etc. are relevant to my present research I will listen.' The point of importance is that McCreery makes predictions; and it seems to me that, if the predicted results are in fact forthcoming, many of us will be forced to take ESP research more seriously than we do at present. As things are, however, his thesis is merely of the 'wouldn't it be interesting if... ?' variety, and it is arguable that the time for publication is usually when the hypothesis has been tested rather than beforehand.

On the general issues of ESP my quarrel is in fact not so much with McCreery as with contemporary fence-sitters. Many psychologists pride themselves nowadays on being 'broad-minded'; and to preserve this image of themselves some of them pay lip-service to the possibility of ESP so as not to seem 'dogmatic'. In my opinion this is muddled thinking. It is plain from a scrutiny of journals that when such psychologists do their ordinary experiments (e.g. on perception or remembering) they do *not* attempt to control for ESP. (How to do so, incidentally, raises all kinds of difficulties, but that is another matter.) Personally I believe that the decision to ignore ESP in one's ordinary research is correct; but in that case it is inconsistent and hypocriticial to say in one's theoretical moments that one regards it as a serious possibility. Professions of open-mindedness are not necessarily a virtue.

Those interested in ESP should certainly read this book. I am unrepentant in my scepticism, but still think it is a good book of its kind.

T. R. MILES

Heredity and Environment in the Functional Psychoses: an Epidemiological–Clinical Twin Study. By EINAR KRINGLEN. London: Heinemann. 1967. Pp. 201. 65*s.*

In 1963 Tienari of Finland published his finding of zero concordance, or diagnostic pair agreement, for male monozygotic schizophrenic twins coupled with the finding of 5 per cent concordance for dizygotics. Psychiatrists, although murmuring against the strongly genetic theories of the aetiology of schizophrenia taught them, were nevertheless shaken by these results, which are so very discrepant from those of German, American and British studies. A second Scandinavian report of low concordance for schizophrenia in male monozygotic Norwegians soon followed and the consequence has been a strengthening of interest in epidemiological twin studies in psychiatry. This monograph by Kringlen (to be followed by a volume of complete case studies) represents a natural extension to his earlier small study and is a detailed report of psychotic concordance and discordance over the whole Norwegian population.

Functional psychotics who were also twins were identified by comparison of records of all Norwegian births 1901–30 and a Central Register of Psychosis. Thus there was a large and unselected sample of psychotic twins—in all, 519 pairs. After exclusion of organic cases and deaths before the age of 15, 342 pairs remained, and for these, careful investigations of zygosity and of psychiatric status have been carried out with extensive social and psychiatric investigations of members of their families, especially the families of the monozygotic twin pairs, by personal interview by the author.

The functional psychoses are subclassified according to Scandinavian practice. British and American workers will find the classification familiar enough, though the 'reactive psychoses' seem to form a group in the borderline between schizophrenia and manic-depressive illness. Both in terms of numbers represented and in terms of interest the emphasis is, of course, on schizophrenia.

Concordance rates for all types of functional psychosis together were found to be 24 per cent in monozygotics and 6 per cent in dizygotics. The figures for schizophrenia were 25–38 per cent for MZ and 4–10 per cent for DZ, which difference supports a genetic factor in the aetiology of schizophrenia. The genetic factor in these results is, however, weaker than it is usually considered to be, and indeed, since the concordance rate in monozygotics is in all studies less than 100 per cent, it must suggest a complex of genetic–environmental interactions. Kringlen finds no evidence for greater genetic determination in one form of schizophrenia than in another, and he concludes in favour of a polygenic form of genetic predisposition interacting with envir ntal factors to account for both the frequency of psychosis in the general population and served concordance rates.

Previous work is admirably reviewed and discrepant findings discussed. Details sent Norwegian findings are in agreement with much previous work, except that the o greater concordance for females than for males. As for the overall concordance figures Gottesman & Shields' suggestion that MZ concordance findings (ranging as they do i cent to 86 per cent) represent a continuum explicable in terms of sampling and other Kringlen's figures of 25–38 per cent may be accepted as both moderate and based on possi most complete and unselected sample yet achieved.

Psychologists will miss the objectivity of measurement when intellectual and educatio levels are discussed. This lack is understandable, however, in a survey conducted by a sole psychiatrist and often by interview in remote households. But we must ask why it is that psychologists are not more frequently and more directly involved in research of such potential. Why, also, in these times should such work be the work of one man when team-work could have many advantages?

Psychogeneticists will deplore the assumption, implicit and explicit, that twin-study constitutes the ultimate in what is possible in human genetic investigations. But sibling and parent data are well covered also by the author. Minor criticisms can be made, but in many respects this investigation is a model of planning, sampling, execution and reporting. Many weaknesses of previous studies are avoided. Remaining weaknesses are plainly recognized and the whole project is discussed with clarity and judgement. Read in conjunction with Gottesman & Shields' recent review, this monograph gives the latest in informed and moderate thinking on the subject of heredity and environment in the functional psychoses.

ANNE BROADHURST

Stimulus Sampling Theory. By EDITH D. NEIMARK and WILLIAM K. ESTES. San Francisco and London: Holden-Day. 1967. Pp. xvi + 681. $11.75.

The dust-jacket describes this book as 'The only recent summary of the entire theory comprehensible to advanced undergraduates, graduate students, and the general non-specialist reader'. Such a summary of the work of the Indiana–Stanford axis is sorely needed by those outside its immediate sphere of influence who would like to apply in their own work the precision and rigour of modern learning theories. It is therefore somewhat disappointing to find that the book consists of reprints of 49 previously published papers, with Professor Neimark providing some continuity in her lucid, but all too brief, introductions to each of the five sections, euphemistically described as 'chapters'.

Anyone who reads the *Psychological Review* and the *Journal of Experimental Psychology* will already have seen 24 of the papers. Nine more are taken from *Psychometrika* and the *Journal of Mathematical Psychology*. The remainder consist of six from other well-known journals and 10 extracts from previously published books on mathematical psychology, mainly from the Stanford University Press. Thus most of the papers come from relatively few sources and are not difficult to obtain. The main virtue of the book is that it rearranges the papers into a logical sequence, rather than in temporal order, and provides a package more portable than the originals.

As one might expect, the individual papers are of a high standard and most of Estes' important work is included.

The first section on general theory is made up mainly from papers by Estes, who starts with the notion of samples of fixed size with equal probability of sampling for each element and leads on to varying sample size with each element having its own sampling probability.

The second section groups together papers dealing with the theoretical framework of associative learning on an all-or-none basis. They include the model proposed by Bower, using a single stimulus element which is sampled on every trial, and the model of Suppes & Ginsberg, in which two stimulus elements associated with each situation have equal probability of one of them being sampled on every trial.

In the third section, on probability learning, the merits of the assumption of equal sampling probability, θ, for each element are compared with the assumption of a different θ for each element. Although various papers suggest that the latter alternative provides a more realistic description of many experimental situations, its adoption would lead to abandoning the assumption of independence of path. This is the assumption that change in response probability from trial n to trial $n+1$ is a function only of the experimental outcome on trial n and is independent of the prior sequence of reinforced trials. Despite doubts about whether this assumption is justified, it proves to be of such mathematical convenience that it is retained in most of the papers in section III dealing with extensions of the theories, and in the following section IV on 'Generalization and Discrimination'.

To the naive experimenter who asks: 'What is a stimulus element?' the reply is that it is not defined in terms of observable events (p. 4). 'It has the status of a primitive term within the theory...To the extent that directly testable implications of the theory are supported by evidence, the concepts of stimulus sampling remain tenable.' But the question cannot be so easily stifled. In section V it appears in a different form, posing one of the knottiest problems for theories of stimulus sampling—the problem of selective, rather than random, sampling of stimulus elements. In this section some ways of achieving such selection are considered. For example, in papers by Atkinson and by Bower, use is made of the orientating response. Finally, one is led to the notion of sampling hypothesis or strategies—the anticipated consequences of alternative responses, although papers on these topics by Estes and others which have been published since 1965 have not been included.

At the end of her introduction Professor Neimark concludes by saying: 'There are easy and difficult routes to any conclusion, the latter often repaying the extra effort by deepened understanding. We have tried to arrange this volume to make the long route available to readers...'

For the benefit of those who falter in following the long route step by step, may one express the hope that someone with the ability and knowledge of Estes or Neimark might prepare a guide to the short-cuts and pitfalls, so that the readers to whom they address themselves may have some chance of keeping in step with them?

R. DAVIS

The Technology of Teaching. By B. F. SKINNER. New York: Appleton-Century-Crofts. 1968. Pp. ix + 280. \$2.95.

This book consists of 11 chapters, seven of which have already been published or read publicly as separate papers; all are written with the author's usual vigour and charm. The unifying and principal theme is the definition of teaching as the arrangement of contingencies of reinforcement. Early chapters expound this theme in terms of pigeons and of 'teaching machines'. Later chapters discuss motivation, thinking, creativity, freedom, discipline and education in general.

In the middle of chapter 4 this reviewer was brought up short by a sentence referring to 'these principles of programming'—evidently some principles are intended to be discernible. Two such principles seem to be that aversive control (punishment) is ineffective, and that natural environmental reinforcement (where the pupil learns by discovery) is less effective than the suitable arrangement of reinforcement by the teacher himself. Much research has been done by others on such questions as the effectiveness of praise *v.* blame, discovery *v.* guidance, overt *v.* covert response, direct *v.* vicarious reinforcement, etc., much of which is relevant to the main and subsidiary themes of this book. Skinner ignores this research, apparently (pp. 243–6) because it involves testing the pupil to find out what he has learnt. Skinner writes of his own research method: 'it seems (*sic*) to have no use for tests which purport to measure what the student has learned' (p. 243). Thus the type of evidence considered is severely restricted; the rationale of this restriction is not explained at all clearly, and we are left to wonder whether Skinner's research method allows any empirical check on his technological principles. The avoidance of empirical hypothesis-testing is perhaps consistent with his view that aversive control is ineffective. We do not learn from our mistakes, only from the reinforcement which follows *correct* responses. We may infer that, in so far as the testing of a hypothesis may lead to its falsification, it is an ineffective method of research.

'A dedicated person is one who remains active for long periods of time without reinforcement' (p. 165). One does not have to be a dedicated Skinnerian to read and enjoy this book; it's his being so cheerful that keeps one going.

W. S. ANTHONY

The Neuroses. By HENRY P. LAUGHLIN. Washington: Butterworth. 1967. Pp. xii+1076. 178*s* 6*d.*

Dr Laughlin, an experienced psychiatrist and former president of the American College of Psychiatrists, has written a successor to his earlier book, *The Neuroses in Clinical Practice*; the book is intended chiefly for the student and the trainee psychiatrist.

Dr Laughlin describes in detail various neurotic syndromes, including rare forms of neurotic reactions, illustrated by case histories; the aetiology and psychodynamics of the syndromes are described and methods of treatment suggested. In addition, the results of several of the author's epidemiological studies are described.

Each chapter is preceded by good historical notes, which trace the development of present views on the subject. The style is lucid. The case histories suggest that the author is a sensitive observer, who empathizes with his patients. The book is very well produced.

In the reviewer's opinion, however, the book has several shortcomings. It is doubtful whether some of the syndromes described merit separate names or categories. The views expressed are largely based on psychoanalytical theories and the evidence in favour of other views is inadequately discussed. The author believes that psychoanalytically orientated, intensive psychotherapy is the appropriate treatment for most neurotic reactions and other methods of treatment are inadequately evaluated. The results of controlled experiments in the treatment of neuroses with psychotherapy and with drugs are not described. The section on drug treatment of neuroses is very short and a few of the commonly used tranquillizers and antidepressants are not even mentioned.

Because of the emphasis on one approach, the book is not recommended for the novice; it is of interest as an exposition of the views and description of the practice of an experienced analytically orientated psychotherapist.

ROBERT KELLNER

The Dynamics of Behaviour Development: an Epigenetic View. By ZING-YANG KUO. New York: Random Press. 1967. Pp. xii+242. $2.45.

Whatever happened to Zing-Yang Kuo? One suddenly realizes that he has been missing from the scene since he inspired a generation of past-war students of psychology to think about the concept of instinct by his experiment that showed how cats could lie down with mice and even rats, and perhaps, therefore, the lion with the lamb. In the 1920s and 1930s he gave promise of being one of the great. Spence pointed to him as a precursor of the drive-reduction theory of reinforcement that Hull was to make so much of later. Now we see Kuo return like some Rip Van

Winkle, making a Chinese bid for a Psychological Peace Prize—to bring together the warring combatants and change them like his former cats by eliciting what he now calls 'behavioural potentials', according to his 'epigenetic' theory, so that those melodramatic characters, the aggressive behaviour geneticists, lie down with their gentler behaviourist environmentalist colleagues. The theory is another attempt at a revolutionary eclecticism of the all-embracing type; and not a bad failure—except that the old (if redefined) epithets like 'epigenetic' will be stimuli for an allergic reaction now endemic among Anglo-Saxon expiricists. Neologisms at the general theoretical level tend to confuse what is often the simple commonsense of positive proposals of this type. In similar fashion any underlying value is lost in Kuo's often naive lambasting of almost all the common concepts of biological discussion (the terms 'innate' and 'learned' are obsolete, even 'natural selection' and 'survival value' are 'dubious'). When his own concepts are fully worked out, he hopes, not only will the old concepts of traditional psychology—such as 'consciousness', 'memory', 'intelligence', 'insight', 'instinct', etc.—become superfluous, but also the current view of 'motivation', 'emotion' and 'learning models'!

All who wish to have an answer to the 'case history' question—the question with which this review began—must read Kuo's disarming introduction to this book, even if their lives are normally too busy to read introductions. In it he claims that he retains the iconoclastic spirit and scepticism that his teacher Tolman attributed to him, but prides himself rightly as having achieved, in maturity, a milder tone. The ambitious claim nevertheless is there and it is that of the Promethean revolutionary, appropriate perhaps as the prime spur to action in the sphere of practical values such as politics, but out of place in science where progress depends upon the accumulation of small devoted acts of construction, or of destruction, such as Kuo achieved in his early papers. Nevertheless, there is much in this manifesto that is interesting, especially in the observations our Rip Van Winkle has made during his 20 yr.-long sleep, but his isolation from the scientific scene leads to some notable informational gaps, particularly in his discussion and references on motivation and on problems of behavioural genetics. At the same time what seems to have been a free individual development gives a headlong and curious originality to his presentation, which it would not have had if he had spent his year 'brainstorming' at Palo Alto.

How 'dynamic' in the history of our science are the 'dynamics of behavioural development?' What are the contributions of epigeneticism to the enhancement of theoretical clarity in psychology?

Kuo's 'Summary and Epilogue', the final chapter of his discussion, consists of 10 sections, the first of which reveal the essential triteness and acceptability of his 'revolutionary' criticism. First, behaviour is more complex than Watson thought. Secondly, 'determining factors' may be 'morphological', 'bio-physico-chemical', 'developmental', 'stimulatory' or 'environmental'. Thirdly, the factors are 'dynamically inter-related'. Fourthly, concepts of structive and acquired behaviour have been oversimplified. The novel ideas adumbrated with a new corollary jargon are given as 'the concept of behavioural gradients' and 'the concept of behavioural potentials' and result in some less than surprising resolutions of problems such as the nature–nature puzzle, namely: 'The actual range of the behavioural repertoire is determined by the history of the animal's development in relation to the environmental context. Because of its anatomy, the Peking duck has the potential to swim, but whether it will actually become a water bird or develop a water phobia depends upon the nature of the environmental context under which the newly hatched duck is reared. Furthermore, restricted by its anatomy, no Peking duck, developed in any environment, will ever become as good a flyer as a wood duck. On the other hand, as a social animal, the Peking duck, within the limitations of its structural and functional capabilities, possessed a relatively wide range of potentials. The actual patterns of its social behaviour are determined by the environmental factors during the developmental history.'

The conclusion of this reviewer is that Kuo's 'epigenetic view' of the 'dynamics of behavioural development', in playing such variations on well-loved themes, is not likely to be world-shattering. The milder Kuo has offered us an over-ambitious composition, not discordant or altogether displeasing, but somewhat confused in its 'dynamic' harmonies, despite some enjoyable echoes of simpler classics.

GEORGE WESTBY

Concepts and the Structure of Memory. Edited by Benjamin Kleinmuntz. New York: Wiley. 1967. Pp. 286. 64*s*.

Many books have mouth-watering titles these days. Often, alas, they consist of a collection of papers of uneven quality bearing little relation either to one another or to the titles. This book consists (mainly) of papers presented at the Second Annual Symposium on Cognition held at the Carnegie Institute of Technology in April 1966. Fortunately, it is well above average for its type. It has been ably edited by Benjamin Kleinmuntz, who contributes a lucid preface. Two chapters have been specially written for the book 'to provide readers... with an integrated overview'. Every third chapter in the book consists of a critical discussion of the two previous chapters: this greatly improves its cohesion. J. G. Holland discusses the chapter by L. E. Bourne on the learning and use of conceptual roles and also that by E. Martin on the role of inhibition in concept formation. He cites Terrace's work on errorless discrimination learning as evidence against assigning a central role to inhibition in concept formation. He seems somewhat dubious of the value of Bourne's work, despite its ingenuity, because of its apparent lack of relevance to concept learning in the child. B. B. Murdock discusses chapters by Earl B. Hunt and L. W. Gregg. The former is an exercise in computer programming of potential rather than actualized relevance to human concept learning. Gregg describes an experiment which essentially concerns serial learning and the strategies which subjects may use. Murdock also provides a brief but useful review of some of the literature on concept formation. The next two chapters are by L. R. Peterson and C. N. Cofer. Peterson describes a number of memory studies and shows a willingness to step beyond traditional associationist concepts in analysing what leads to an overt attempt at recall. A. W. Melton, who reviews this paper, admits that it 'touched a sensitive nerve (some might call it a soft spot) in an old associationist'. While not wholly rejecting Peterson's notion of the editing of implicit responses, he argues that the original coding of the input is likely to limit the pool of implicit responses and reduce the need for editing. Melton's chapter, which includes brief references to Cofer's paper (concerned with conceptual organization and free recall), is worth reading as an up-to-date statement of the associationist position. The remaining chapters are those specially written for the book. C. F. Nodine examines the role of temporal variables in the acquisition of concepts. A. Newall & H. A. Simon provide an exceptionally readable final chapter, in which they make insightful comments on nearly all the other contributions. In saying this, I reveal my own bias in favour of viewing man as an information-processing system. It is nice to find in their chapter a reference to Bartlett's *Remembering*, it is nice to be reminded that LTM contains programs, i.e. schema for responding rather than responses, and refreshing to be told that 'the larder of possible mechanisms should be well stocked before we consider which one or ones is supported by data'. This is a book I thoroughly recommend.

JOHN BROWN

Automation: its Anatomy and Physiology. By J. Rose. Edinburgh and London: Oliver & Boyd. 1967. Pp. vii+144. 7*s*. 6*d*.

Automation: its Uses and Consequences. By J. Rose. Edinburgh and London: Oliver & Boyd. 1967. Pp. vii+123. 7*s*. 6*d*.

These two companion volumes by the same author appear as nos. 8 and 9 in the 'Contemporary Science Paperbacks' series. Between them, they set out to present an elementary account of the basic principles of computers, of the theory of control and of the managerial consequences arising from the advent of automation. By and large the author achieves his purposes; the reader is presented with a good deal of information about both hardware and software systems, and is offered an intelligent appraisal of the economic and social implications of recent technological innovations in the handling of information.

The former volume deals with the technology of automation, and keeps the reader historically orientated throughout. The material is divided into three sections. The first is concerned mainly with computers, the second with feedback control theory, and the third with some examples of control applications. The author does not shy away from a little mathematics when this is appropriate (e.g. the section on stability criteria), but little is lost if these sections are avoided.

The text is liberally supported with diagrams and tables; there is a useful glossary of terms and bilbiography.

The first chapter (and nearly half) of the latter volume is essentially a continuation of the technological survey. Here, much more is said about the practical details of automatic process control and data processing. The following chapter discusses the capital and recurrent costs of automation, and the repercussions of its introduction upon the labour market. The author then moves on to an account of human attitudes in the face of innovation, and touches upon the problems of educating both sides of industry to cope with the change. A short final section is given over to the inevitable crystal ball. Again, a good bibliography is provided.

There are many less adequate ways of obtaining an introductory understanding of what automation is all about.

ELWYN EDWARDS

Cognitive Psychology. By ULRICH NEISSER. New York: Appleton-Century-Crofts. 1967. Pp. xi + 351. $7.50.

This book's basic attraction is the quality of thought behind it. Through most of it, Neisser writes lucidly and penetratingly on issues about which he has obviously thought honestly and deeply. This is not to say that all his interpretations are undebatable, that the wood always stands out from the trees, or that the book would suffer by being pruned in places. But here is fresh directness of consideration, unencumbered by those crippling, superficial, and unacknowledged presuppositions which are too familiar in psychology; and here is concern for basic organizational properties of human function. In these respects, and in its subject-matter, the book invites comparison with Hebb's *Organization of Behaviour* (1949) and Broadbent's *Perception and Communication* (1958).

Readers of this intelligent book should be warned that its range is narrower than its title suggests. Its more fitting title might be 'Seeing and hearing as constructive activities', for it is largely devoted to detailed considerations of fairly recent experiments about visual and auditory perceiving and attending—or, as Neisser would say, visual and auditory cognition. Neisser is already well known for his elegant experiments about visual search and recognition, and he is clearly interested in working toward those activities, such as thinking, reasoning, imagining, which are commonly given the generic name of cognitive processes. However, his present intention is not explicitly to consider such higher-level activities but to pursue a 'cognitive approach' to what would usually be called perception, and hopefully to indicate wider implications of this approach. In particular, he aims to flesh out the ramifications of two broad assertions. The first is that instances of perceiving are akin to higher mental processes in that, however simple and passive they might seem, they are essentially constructive; the active perceiver *uses* sensory information. Here Neisser acknowledges the ideas of, among others, Bartlett; and he frequently cites Hebb's comparison of the perceiver with the palaeontologist who extracts fragments of what might be bones from a mass of irrelevant rubble and constructs a dinosaur skeleton. The second assertion is that, in this multi-stage construction, there are two kinds of stages with contrasting properties: 'the first is fast, crude, wholistic, and parallel while the second is deliberate, attentive, detailed, and sequential' (p. 10). These two assertions are the twin themes of the book. Both have respectable lineage in the history of psychology, and both concern the centrally organized nature of psychological man. They give Neisser some continuity for his detailed reviews of various experimental issues, and they justify his final, rather abstract, chapter on memory and thought.

There are two reasons for welcoming this book. First, it is a competent, clearly written, up-to-date reference for such topics as: tachistoscopic experiments, with their implications for what Neisser calls iconic (or in the case of hearing, echoic) memory, and for coding from such memory; visual pattern recognition and search, with their theories of template-matching *v.* feature-analysis; pre-attentive and attentive processes, and figural synthesis; word-recognition and speech perception; short-term memory; visual imaging and dreaming. At the present time, such topics attract much research, and readers who want to know the state of play could fare worse than refer to Neisser. Secondly, the book is more than a critical examination of currently fashionable research: it advances a general constructionist viewpoint which has run importantly through psychology for some centuries and has been more neglected than it deserves. This viewpoint has been sketchily indicated above and, in the book, it is elaborated by some fine theoretical discussion and evaluated against a variety of hard factual findings. The viewpoint emerges, without polemics, as both comprehensive and plausible, and it shows promise for synthesizing, not only the range of perceptual work considered here, but also much other work on the organization and development of intellectual functioning as well as the insights of dynamic psychology. All this should make readers wish that Neisser had explicitly explored further topics, and make them look forward to the future volume which is promised. In the meantime, this book is, in the words of Bacon, to be chewed and digested.

I. M. L. HUNTER

Dogmatism and Visual Loss. By PHYLLIS N. HALLENBECK. (Research Series, no. 17.) New York: American Foundation for the Blind. 1967. Pp. x+108. $1.50.

This little book presents the results of an attempt to apply Rokeach's theory of belief systems to the reactions of 32 blind men on first encountering rehabilitation services to the United States. It is a Ph.D. thesis completed in 1964—a mere 4 yr. after the publication of *The Open and Closed Mind.* It purports to be a contribution to the clinical problem of adjustment to blindness. The usefulness of the work must therefore be judged by two rather separate sets of criteria. 'It is the contention that severe disability is a special case of bereavement involving the permanent loss of important parts or functions. One loves one's body and reacts to disability as a personal loss. Are the reactions to bereavement similar to the reactions to severe disability?' (p. 1). It is held by the author that such bereavement requires a change in one's system of belief and the extent to which this is achieved by the patient is measured by scales of 'dogmatism', 'denial' and 'depression'. Certain hypotheses about the relations between scores on these scales are set up and sustained by significant correlations, ranging from 0·42 to 0·75; this makes them theoretically interesting, but of doubtful immediate practical value.

The main value of the book lies in the detailed excerpts from the various protocols which are used to fill in the bare bones of the correlational findings. This material goes some way towards arriving at an understanding of the author's belief system about the function of rehabilitation and of her attitudes towards disability. The credo is a misunderstood version of the reality principle. The replies of patients, who fail to face what the author accepts as their reality, are handled in a manner both derogatory and without compassion. There is no attempt to show what constitutes good and poor ultimate adjustments, and little sign of understanding the problems of either patients or rehabilitation staff. Perhaps there is nothing wrong at all in a patient believing firmly that one day his sight may be restored even if all the doctors in the world agree on a contrary view, as long as that belief does not prevent him from learning new skills which can help him to lead a useful life. For all we know, this form of belief may help to deal effectively with the grimness of the present reality. Perhaps the greatest skill in rehabilitation is shown by those who enable their patients to sustain what appear to be logically inconsistent beliefs. But until we know more, much more about this, one should be very careful to generalize from 32 subjects' responses to dogmatism, denial and depression.

The laboratory experiment is always a game in the sense that in the last resort the subject rarely needs to take a real risk or is exposed to stress with irreversible consequences. It is admirable and desirable for all sorts of reasons to do research in real settings as long as one remembers that frequently the relevant variable constitutes a condition under which the subjects have to live for long after the results of the study have been written up. Those of us who work with the disabled must learn to be both humble and compassionate, as well as open-minded.

J. ALFRED LEONARD

Four Years Old in an Urban Community. By JOHN NEWSON and ELIZABETH NEWSON. London: Allen & Unwin. 1968. Pp. 570. 60*s*.

This is the second report of a longitudinal study of the way parents bring up their children in a Midland city. Like the first, published in 1963 under the title of *Infant Care in an Urban Community,* it is based upon the intensive interviewing of a sample of 700 mothers, this time of 4-yr.-old children. For various reasons it was only found possible to include in the second survey

275 of the subjects interviewed in the first, though in subsequent reports it is hoped that a 100 per cent follow-up will be maintained.

The book opens with a short but spirited defence of the method adopted. Mothers are the experts on their own children in the sense that they know more about them, and in more situations, than anyone else; given optimal interview conditions and a sympathetic and non-evaluative interviewer, it is reasonable to suppose they will be as honest and as accurate as they can; it is therefore obviously sensible to exploit their knowledge by asking them. From the results, the mothers plainly relished the opportunity to talk at length, a tribute to the skill of the interviewers and to the mothers' preoccupation with their children. On the assumption that 'specific practices in child care...are a good deal less important in the long run than the spirit in which they are carried out', the interviews were designed to yield, not only factual information about child and parent behaviour, but also evidence of the emotional context within which alone such behaviour can be properly evaluated. To the objection that the mother's impression of her child's behaviour may be very different from that of an 'objective' observer, the authors would doubtless reply that because she is, at this age, the main socializing agent, and because the focus of the research is on the mother–child relationship, an understanding of her perspective is more important than any other, and it is one which has hitherto been neglected.

The main body of the book consists of chapters dealing with independence, control of aggression, privacy, behaviour at mealtime and bedtime, rituals and comfort habits, toilet-training, modesty, discipline and telling the truth. Each contains statistical tabulations of data, numerous quotations from the interview protocols, and much perceptive comment in which the meaning of behaviours is found in the attitudes the mothers expressed and in the full context in which they occurred. As a result the reader is made vividly aware of the personalities of the mothers, their perplexities and devotion, so that he cannot but share the authors' evident respect for them.

However, the psychologist in search of antecedent–consequent covariances will have his appetite whetted, but for its satisfaction will have to wait impatiently for further publications. Most of the book is given over to a descriptive account of what the mothers and children are like and how they behave to each other; aside from the ubiquitous class differences, relatively little space is devoted to either the interrelations between variables or the relations between the present data and the earlier survey. As for the former, findings such as that maternal pressure for independence is related to actual independent behaviour, and that the mother's belief in the efficacy of smacking is positively correlated with the extent to which she smacks, raise obvious problems of interpretation when the mother is the source of measurement for both variables. The authors are of course aware of this, and from their discussion it is clear that the issue is too complex to be settled in general terms, but that each association must be appraised independently in the light of the questions asked and the conditions of the interview. As for the latter, the most interesting finding is that not one of the measures of maternal attitude and practice with regard to toilet-training which were obtained when the child was 12 months old was predictive of the difficulties experienced by the child at 4. There is no evidence here that early toilet-training had undesirable consequences.

This is an important book, written with clarity and verve, and absorbing to read. For the developmental psychologist, it brings home unequivocally the need to control for class difference in studies of social development, and, because the authors have not hesitated to engage in speculative explanations of their results, it provides a rich source of hypotheses for testing in more limited and controlled research designs.

DEREK WRIGHT

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